NATURE

No. 4479 SATURDAY, SEPTEMBER 3, 1955 Vol. 176

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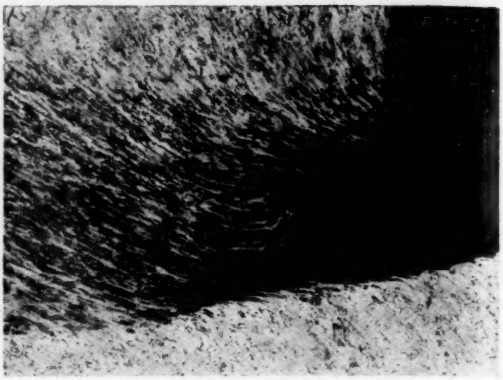

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THE INTRINSIC VALUE OF SCIENCE

THE title of Sir Robert Robinson's presidential address to the British Association, which is printed in full beginning on p. 433, recalls the exposition of the cultural and human value of science which a former president, Sir Edward Appleton, gave to the Association in Liverpool two years ago. The similarity indeed goes further. Both addresses provoke or invite discussion on such topics as the teaching and training of scientists and the place of science in, and its use as, a vehicle of liberal education. But though Sir Robert displays to the full, as did Sir Edward Appleton, the place of imagination as well as of analysis and order in the advance of science, his main concern is with the scientist. As Sir Edward testified to the cultural and human value of science, so Sir Robert presents the scientist as a human person and displays the conditions under which he goes to work, his dignity and his problems, stripped of the mystery or magic with which popular opinion is all too prone to invest him, and which in this age of technology can be so fraught with danger to mankind.

Sir Robert expressly excludes scientific method itself from his observations; but there are two points in connexion with the definition of science with which he starts that are worth emphasizing in view of this tendency to attribute mystical or magical powers to the scientist. Science, he insists, is simply the record of the system of knowledge and of the conceptions correlating parts of that system. That record, he points out, may be imperfect and the conceptions erroneous. Accordingly, science is potentially fallible, and accordingly also it cannot be based on dogma or authority of any kind.

While Sir Robert adds that there are degrees of uncertainty and that even the apparent revolution in physics which has occurred in the twentieth century has not so much superseded the older teaching as supplemented and extended it by wider generalization, this first point as to the limitations of science is of fundamental importance in regard to the position of the man of science in society. Later in his address, in referring to nuclear explosions and the confident claim of the physicists that there is no risk of unpredictable reactions in the lighter elements if nuclear tests are taken to further stages, Sir Robert wonders whether we really know enough about nuclear reactions to be sure that there is no loophole, and whether every conceivable case has been considered. Cromwell's plea to the General Assembly of the Church of Scotland, "I beseech you, by the bowels of Christ think it possible you may be mistaken", might well be emblazoned on the walls of every research laboratory; and it is the realization among scientists themselves of the limits of our knowledge that has found expression recently in their misgivings as to the continuance of nuclear test explosions and the biological hazards of radiation.

The second point is also well brought out by Sir Robert at a later stage in his address. After remarking on the improvement of knowledge by successive approximations, he proceeds to illustrate his point by selecting examples from the history of science with the imaginative skill which is the mark of the artist no less than of the creative scientist. It is not, however, so much the way that scientific theory is justified by the success of predictions and by the unanticipated convergence of more than one line of approach that are impressive. Far more important than the fact that the degree of coincidence may become so great as to carry conviction that we have been vouchsafed, if not the whole truth, at least something very closely related to it, is the fact that this whole process depends on freedom of communication. "Who ever", cried Milton in the Areopagitica, "knew Truth to be put to the worse, in a free and open encounter". The principle of free discussion is implicit in the conception of science, and it is the birthright of the scientist.

Probably the most important feature of the Geneva Conference on the Peaceful Uses of Atomic Energy is the demonstration it gave of just that relaxation of secrecy on the scientific aspects of the development of nuclear energy to which Sir Robert refers. The five hundred technical papers read at that Conference discussed details which were close secrets only a short time ago. Now the fruits of some fifteen years of intensive, laborious and costly research are becoming largely international property, with benefits to further advances and advantages in economy of scientific and technical man-power and resources that can scarcely be over-stressed.

Sir Robert Robinson also suggests some form of international consultation before novel and greatly increased releases of energy are attempted. This lay outside the strictly technical orbit of the Geneva Conference; but the very success of the Geneva Conference in discussing matters previously so largely secret led its members to realize that it might soon be necessary for some body—and not a scientific body—to make proper arrangements for administering the solutions to the problems of the nuclear age to which technical advance might point. That in itself is a factor indicating the need for clearer understanding of the scientist and technologist as men, and of the significance of the technical and scientific elements in a political, social or economic problem, on the part of the administrator, whether the latter be Minister or Civil servant.

Meanwhile, however, it is all to the good that the importance of this relaxation of secrecy should be stressed so authoritatively; and scientists generally can show their appreciation of Sir Robert Robinson's address in no more fitting way than by emphasizing at every appropriate opportunity the vital connexion between scientific advance and the free communication and discussion of results and ideas. That this has been so admirably stated on repeated occasions in the past decade—by Sir Henry Dale and by the Commission on Higher Education in the United States, for example—does not minimize

the need for continuing this process of educating the public understanding of the prime importance to a free society of academic freedom and untrammelled avenues of communication and discussion. The larger the place science and technology come to occupy in society, the greater the importance to society itself that the scientist should be able to move with freedom toward the truth wherever the path may lead, stayed by no authority or tradition, and hindered by no dogma, prejudice or vested interest from thoroughly exploring every promising avenue he sees. The whole atmosphere of the Geneva Conference warrants the hope that the climate of opinion is changing, and that statesmen and the public generally are disposed to accept an argument that for ten years or more has largely been rejected. To seize that opportunity is not solely a matter for the man of science. Rightly and firmly, Sir Robert includes all branches of knowledge in his definition of science, provided their record has been made without appeal to authority, nor does he believe that any philosophical line of demarcation separates one branch of learning from another.

Here he is at one with J. B. Atkins, who in "Incidents and Reflections" pleaded for the universal recognition that physical science belongs in essence to the humanities, and is a part, and may become the most important part, of that body of teaching, which though once narrowly confined was always destined to expand and in unity embrace the obligations of the good life. Emphatically, too, Sir Robert rejects the assertion that culture is the preserve of students of some particular branches of knowledge and is denied to others. Moreover, while the illustrations he chose to exemplify his thesis show he concurs with Sir Howard Florey's remark, in his Lister Memorial Lecture, that real and continuous progress and understanding in such a field as that of antibiotics research can be obtained only by the collaboration of people with various outlooks and experimental accomplishments, it is clear that Sir Robert recognizes that contributions to scientific advance can come from people whose primary occupation is not usually regarded as scientific.

Sir Robert's skilful exposition of the nature and drama of the scientific discipline may well induce the passing reflexion that a wider and wiser use of the historical method in teaching science might contribute not merely to give the scientist a clearer perspective in his work but also, and perhaps more important still, to give the non-scientist an understanding of the mind and method of the scientist and the conditions necessary for effective scientific work, as well as the limitations of that work. So too in his reference to the universality and the fully international character of science, Sir Robert indicates both the reality of the brotherhood of scientists—which of course is by no means unique—and the real basis for the state of mind which has found expression in Lord Russell's statement and others on the problems of the release of atomic energy. He sees in the world-wide co-operation which the development of science requires, one of the bulwarks of peace.

There are passages in Sir Robert's address which testify to the intrinsic value of science as fully as did Sir Edward Appleton in his presidential address at Liverpool. They recall the view of science which Eddington advanced in a Gifford Lecture as springing from "a striving which the mind is impelled to follow, a questioning that will not be suppressed". But by and large, it is in what he reveals of himself in this address, more than in what he said, that Sir Robert displays the human qualities of the man of science to the world, and the rich contribution he can bring not merely to its material life and technology but also to its thought and experience. The flashing asides, which uncover fresh angles of discussion on varied topics, such as the need for a technological grants committee to administer technological education, the control of nuclear energy developments, or the training of scientists, do not distract us from the consciousness that in himself the President exemplifies the judgment, the sense of values and the skilful selection that are the mark of a man of the highest culture. If by culture we mean the ability to think clearly and to express those thoughts in language both dignified and lucid, we need look no further for evidence that those trained in the physical or biological sciences possess those qualities as fully as any of their fellows, within or without the walls of a university.

CHRISTIAN FAITH IN THE WORLD OF SCIENCE

Science and Christian Belief

By Prof. C. A. Coulson. (The John Calvin McNair Lectures, 1954.) Pp. x+127. (London: Oxford University Press, 1955.) 8*s*. 6*d*. net.

IN these lectures, delivered at the University of Chapel Hill, North Carolina, Prof. C. A. Coulson has followed up the line of thought set out in his Riddell Lectures in Durham and his address to the British Association at Oxford. He has been singularly successful in interpreting the relationship between the scientific discipline and outlook and that of religion and of the Christian faith. In defining religion as "the total response of man to all his environment" (p. 83) and by insisting that the scientist is no mere gadget-monger but a fully developed man using not only his curiosity and logic but also his imagination, his reflexion and his power of commitment, he can present his case that science represents, as it were, an architect's plan of the building, a plan which gives a true picture of one aspect of the whole but must not claim to be more than this—a plan to be supplemented by other interpretations and harmonized with them (pp. 66–68).

Prof. Coulson begins with a searching criticism of most of the traditional attempts to relate science to religion. His examination of the supposed antithesis, of the attempts to define the intrusions of the one world into the other, of the well-meant desire to find room for God in the gaps left by the incompleteness of the scientific picture, should be studied by every theologian. It is illustrated by a large number of actual examples which show how very

many of the apologists for religion contradict their own professed belief in the omnipresence of God, and treat the physical world as external to Him.

In his second lecture he disposes of the idea that science is merely technology—a particular and strictly limited exercise of the logical intellect. Here again he brings together a great mass of quotations to prove that science, like religion, is a proper activity of the whole man, an adventure demanding full use of all the faculties and inspired by a belief in the unity and coherence of all reality.

In the third lecture he contends that not only is the whole man involved in the scientific quest but also that this quest is ultimately religious in character and linked with other modes of human response to the universe. It is an essential contribution, not only to man's understanding of his environment or to his control of it, but also to his whole reaction to it and experience of it.

Finally, he concludes that this environment is essentially personal or superpersonal in character and that it can therefore be appropriately expressed for humanity "in one person, a representative person, an archetype" (p. 112), and that this person is for the Christian the Christ. The scope and significance of this conviction he states briefly, clearly and with relevant evidence.

It is difficult for one whose general position is very close to that of this book to express anything but admiration for it. If there is to be criticism, it will be that the wealth of quotations, though impressive and well chosen, leaves too little room for Prof. Coulson's own admirably lucid and effective expositions. It is to be hoped that he will before long give us a fuller account of the position here outlined as he sees it in regard not only to the present findings of science but also to the great issues which to-day confront mankind.

CHARLES E. RAVEN

REDBRICK'S DESTINY

Civic University

Aspects of a British Tradition. By W. H. G. Armytage. Pp. 328+8 plates. (London: Ernest Benn, Ltd., 1955.) 30*s*. net.

THE sub-title of Prof. Armytage's book is "Aspects of a British Tradition", and those words provide the clue to his design and its fulfilment. He begins right back with the earliest impacts of Christianity on Britain. In separate chapters he works through the foundation of universities at Oxford and Cambridge, the laicization of the professions, the emergence of a scientific spirit, the contribution of Francis Bacon, the influence of the Royal Society, the rise of corporate patronage, and the nineteenth-century "new clerisy". Then, through the conflict of ideas occasioned by the Great Exhibition of 1851, we arrive at the efflorescence of civic university colleges in England. In the eleventh of the book's fourteen chapters we enter the twentieth century, and the last fifty pages give a clear picture of the problems which confront the universities of the British Isles to-day.

The main body of the book, then, is historical, not argumentative or theoretical. Each chapter is both an essay in itself and a contribution to the developing story; and each stage in the development of the tradition is documented with a wealth of detail. Indeed, in places the detail becomes so rich that the ordinary reader may be overwhelmed by it, for the accumulation of proper names sometimes clogs the march of the narrative.

Prof. Armytage's general thesis is that the transmission of knowledge demands, in each separate historical context, its own appropriate institutions; that the pushing forward of the frontiers of knowledge is not always best done by institutions suitable for its preservation and transmission; and that the demands of science, pure and applied, require for their satisfaction instruments quite different from the universities of tradition. He sees the modern civic universities, in close alliance with technical and technological colleges and with other research-organizing bodies, as the agents of these vital developments; and his tracing of the historical story is designed to show that this is their destiny.

The fascinating narrative which he unfolds inevitably gives rise to questions. The first is the ancient one, of the nature and definition of 'university' itself. It is not to be expected that the opinions of Walter de Merton, Richard Mulcaster, Cardinal Newman, Abraham Flexner and Bruce Truscot should coincide. But more than once a doubt arises whether Prof. Armytage is not too hospitable to quotations from writers who mean by it simply an institution for higher education whether general, specialized, vocational, scientific or commercial; and this multiplicity of meanings darkens counsel.

A second is whether there really is any genuine historical connexion between scientific and technological studies, on one hand, and, on the other, the kind of institution which we know as 'Redbrick University'. There is no doubt that during their slothful centuries Oxford and Cambridge failed to realize their duties and their opportunities in relation to the new knowledge; and there is no doubt either that within the past hundred years needs which Prof. Armytage cannot avoid calling "banausic" have led to the deliberate inclusion within the modern universities of studies which the more ancient ones found indigestible. But it is difficult to believe that there is a necessary connexion. The older universities are by now scarcely less interested in technology than their younger sisters; and of the population of these latter nearly one half are engaged on arts courses.

A third question concerns the extent to which the newer universities ought to become, or remain, provincial, regional or civic. 'Civic' suggests a relationship with the local civic authorities much more intimate than is customary. 'Regional' suggests a connexion with a particular geographical area, and while this may properly give to a university certain special interests it ought not to determine that university's development. Especially, this local connexion ought not to determine the area of recruitment of its students—'provincial' in that sense is an undesirable adjective. More and more the value of life in a hall of residence is recognized, and although there may be something to be said for concentrating various technological studies in the geographically appropriate places, there is much more to be said for collecting the students from as far afield as may be.

There remain the three most urgent major problems of university policy: the possibility of some genuine marriage inside the university course between the new scientific studies and the wider humanities of tradition; the extent to which the clamant national need for educated technologists is for the universities

to supply; and the assessment of the right proportion of university places to the population of the British Isles. Each of these is a matter of daily debate, and to each of them so far the attitude has been empirical, both in the universities and in the nation at large. The growth of the universities during the past fifteen years has been astonishing, not only in the sheer numbers of students and staff, but also in the variety and complexity of the fields of study and investigation. It is natural enough that among its results there should be a certain confusion about aims and untidiness about means. To what extent should the spontaneous and organic development of the individual universities be controlled by some central plan? If at all, how far can central planning avoid infringement of that autonomy which universities have traditionally claimed? Fundamentally, are universities properly regarded as 'community service stations'? These problems Prof. Armytage sets out with clarity and vigour in his concluding chapters.

In fact, this is several books. It is a valuable narrative of the development of many kinds of institution for higher education, including the Inns of Court, the hospitals and the nonconformist academies. It is a series of historical vignettes of particularly fruitful moments in the story of British education. It is a reasoned and well-documented appreciation of the present needs both of the universities and of the society which supports them. Prof. Armytage would not claim that he can prescribe a remedy for our present headaches; but he has certainly given us the material for seeing ourselves in the perspective of a majestically unfolding story.

J. F. Wolfenden

A BIBLIOGRAPHY OF THE WORKS OF LAVOISIER

A Bibliography of the Works of Antoine Laurent Lavoisier, 1743–1794

By Denis I. Duveen and Herbert S. Klickstein. Pp. xxiii+493+42 plates. (London: Wm. Dawson and Sons, Ltd., and E. Weil, 1954.) £10 10*s*.

STUDENTS of Lavoisier have hitherto been badly handicapped by the lack of any adequate bibliography of his writings, which cover so many fields in addition to chemistry. Their difficulties were increased since Lavoisier not infrequently did not publish the papers which he had read to the Academy of Sciences in Paris, while the editors of the six volumes of his collected works, the "Œuvres de Lavoisier", had access to his manuscripts and printed them without any indication of whether they had been previously published or not. So there are many traps for the unwary. For example, the account of Lavoisier's earliest scientific investigation, describing his observations of an aurora, was written in 1763 but was not published until 1863.

Mr. Denis Duveen has been an assiduous and discerning collector of everything relating to Lavoisier, and now, with the aid of his unique collection, he and Dr. H. S. Klickstein have given us this comprehensive volume of bibliographical studies, which is the result of years of research and scholarship. Following the modern tradition of 'personalized bibliographies', it contains not only the bibliographical details of all Lavoisier's books and papers and their translations but also a short analysis and appraisal of their contents which will be of the greatest help to students in the future. With the exception of one or two foreign works, copies of all the 705 printed documents catalogued in the bibliography are in Mr. Duveen's private collection, part of which was shown at an exhibition at the Grolier Club of New York in 1952.

The bibliography contains six sections dealing, respectively, with (1) Lavoisier's papers published in scientific periodicals, (2) his four major works on chemistry, (3) his minor writings which were published separately, (4) his minor contributions to other works, (5) the reports of the committees of the Academy which were written by him, and (6) the various collected editions of his papers and books.

Lavoisier contributed more than a hundred papers to scientific periodicals, and many of those dealing with his more important chemical discoveries appeared in the Memoires de l'Academie Royale des Sciences, the volumes of which were usually published several years in arrears. This has caused historians no little difficulty. Papers published by the Academy are listed in the bibliography, not under the years in which the volume containing them was actually published, but under the years which the particular volume covers. This corresponds in most cases, but not in all, with the date of the reading of the paper. A short summary of each paper is given and a reference to its inclusion in the "Œuvres de Lavoisier" which is most useful.

The second section, dealing with his four major works, the "Opuscules", the "Nomenclature" and the classic "Traité Elémentaire", contains an interesting account of the little-known "Memoires de Chimie", the fragments of the complete edition of his memoirs which Lavoisier had just begun before he was guillotined. These were never put on sale but distributed privately by Madame Lavoisier in 1805.

Sections (3) and (4) contain most of Lavoisier's papers on agriculture, political economy and education. Section (5) deals with the reports written by Lavoisier for committees of the Academy appointed to review books submitted for publication with the approval of that august body; and Section (6) with the various collected editions of his writings, including most useful details of the contents of the six volumes of the "Œuvres" with cross-references to papers already listed in the bibliography and an analysis of those which only appeared in the "Œuvres".

It is impossible in a short review to do justice to the accuracy of detail and the fine scholarship of this bibliography. Those who have had experience of the difficulty of tracing the exact course of Lavoisier's ideas will appreciate the immense amount of study it has involved, and future historians of chemistry will often feel grateful to the authors.

There is one serious omission—a general index, by means of which it would be possible to find immediately any individual paper or to trace groups of papers on particular subjects. There are indexes of names, publishers, printers and artists; but the lack of a general index does diminish seriously the availability of the results of this unique study of Lavoisier's writings. It would be quite easy to remedy this defect in a short supplementary volume, when the opportunity might be taken to include a list of the biographies of Lavoisier and the major papers devoted to the study of his work on the lines of Dr. Holmberg's excellent bibliography of Berzelius.

Looking through this book, with its interesting facsimiles of title-pages, one is struck once again with the amazing range of Lavoisier's accomplishment in his short life of fifty years. In chemistry, in physics, in physiology, in agriculture, in geology, in education, in statistics and in finance he was a pioneer, and in each his contribution had the modern touch both in thought and phrase. With the publication of his letters in the near future and of his note-books, historians will have much fresh material, and Mr. Duveen and Dr. Klickstein's bibliography will be of immense value to them in their studies.

HAROLD HARTLEY

VICTOR VAN STRAELEN

Volume Jubilaire—Victor Van Straelen

Directeur de l'Institut royal des Sciences naturelles de Belgique, 1925–1954. Tome 1: Pp. viii + 640 (40 plates). Tome 2: Pp. vi + 641–1216 (8 plates). (Bruxelles: l'Institut royal des Sciences naturelles de Belgique, 1954.) n.p.

PROF. VAN STRAELEN, who was born at Antwerp in 1889, retired in June 1954 from his post as director of the Institut royal des Sciences naturelles de Belgique which he had occupied for twenty-eight years; at a ceremony of "Manifestation d'Hommage" held in his honour shortly afterwards he was presented with two "Volumes Jubilaires" containing contributions from all his scientific staff.

Prof. Van Straelen has had a most distinguished career, in the course of which, following the path of his illustrious predecessors, he has guided the study of the biological sciences in Belgium and the development of the Institut to a position of world-wide esteem and reputation. After a brilliant university career, followed by service in the Belgian forces during the First World War, he occupied various academic posts until at the early age of thirty-five he was appointed director of the Musée royal d'Histoire naturelle de Belgique, which has since become the Institut royal des Sciences naturelles de Belgique.

Van Straelen is primarily a palæontologist, and was particularly distinguished for his researches on the fossil Mollusca and Crustacea; but after this appointment he devoted his great energies and administrative abilities to developing the museum and widening its activities, and to encouraging the study of biology in Belgium. The results of his life's work are manifest in the Institut. Large though the museum was when he went to it in 1925, it is now greatly increased in size by a range of new and imposing buildings which are, however, but a part of the plan that he devised for making the housing of the Institut one of the finest buildings in Europe. It is much to be hoped that the plan will be completed before many years have passed. His scientific staff has grown in numbers from five to twenty-seven, and the important collections of the museum have been enormously enriched during his tenure of office. The publications of the Institut have been greatly multiplied. The well-known *Annales* and the *Mémoires* have been increased in frequency of appearance and augmented by the addition of a second series and the publication of many volumes *hors série*; in addition, the publication of the *Bulletin* was renewed after a lapse of forty-two years. These are only a few of the many changes wrought by Prof. Van Straelen, who has also been very active in furthering the cause of education in the biological sciences in Belgium.

Prof. Van Straelen is a man of great strength of character who steadfastly refused any form of collaboration during the occupation of his country, and whose eminence has been recognized by the conferment of scientific honours throughout the world. He is a foreign fellow of the Linnean, Zoological and Geological Societies of London, foreign or honorary member of a great number of scientific societies in many other countries, and president of the International Association for the Protection of Nature. In Belgium, among many other scientific activities, he is president of the Institut des Parcs nationaux du Congo Belge, president of the Classe des Sciences of the Academie royale des Sciences, des Lettres et des Beaux-Arts de Belgique, and president of the Association pour l'Étude de la Paléontologie et de la Stratigraphie houillères; he has been responsible for the organization of numerous exploring expeditions to the Belgian territories in Africa.

The articles published in the Victor Van Straelen "Volumes Jubilaires" have been contributed by members of the scientific staff of the Institut royal des Sciences naturelles de Belgique, the Association pour l'Étude de la Paléontologie et de la Stratigraphie houillères, and the Institut des Parcs nationaux du Congo Belge. They represent some of the numerous lines of research that he has encouraged or developed in the course of his career. The volumes are beautifully produced and lavishly illustrated, thanks to the generosity of an anonymous benefactor.

Vol. 1 contains papers on mineralogy, palæobotany and palæozoology. They deal largely with the coal-measures of Belgium and include studies on their flora and fauna. In the section on mineralogy, J. Scheere gives a lengthy lithological study of the rocks of the Upper Westphalian *A* series overlying the coal-measures of the Campine, and R. Van Tassel details the occurrence and characters of the arsenopyrite mineral mispickel in Brabant. In the section on palæobotany, F. Stockmans and Y. Willière summarize their previous studies on the Namurian flora of Belgium, and R. Vanhoorne contributes a short paper on the dating of certain peat deposits by a study of the seeds and pollen recovered from them. In the section on palæozoology, M. Lecompte describes the 'reefs' of pink marble found in the Devonian of the Ardennes and discusses their probable formation by stromatoporids in superficial zones of turbulence and by corals at greater depths; and P. Sartenaer discusses the occurrence of two species of brachiopods from Senzeille and describes one of them as new. A short paper by F. Demanet follows, reporting a re-examination of the schistes of the Anhée coalfield; and in the next paper H. Chaudoir tabulates in detail the stratigraphical distribution of the flora and fauna of the eastern part of the Liège coalfield syncline. A. Pastiels contributes a biometric study of some of the lamellibranchs of the Liège basin, in which a number of species are shown to be divisible into two groups which show little overlap in their dimensions. A long paper by M. Glibert and J. de Heinzelin de Braucourt gives a review of the lower oligocene fauna and flora of Belgium, with illustrations of numerous species of mollusca. The next paper goes farther afield with a study of the lignite-bearing miocene strata and the marine pleistocene beds of eastern Tunisia by W. Van Leckwijck. The volume concludes with papers on

the growth of brachiopods of the family Spiriferidae by A. Vandercammen, on some adult fossil Ephemeroptera by G. Demoulin, and an essay on the palæobiogeography of the Euselachii by E. Casier.

Vol. 2 is devoted to zoology, and more than half of it deals with invertebrates. The first paper is a long essay by E. Leloup on the structure and phylogeny of the siphonophores, in which he concludes that these organisms are derived from littoral gymnoblastic hydrozoa, allied to the Tubulariidae, which have been freed from a sessile benthonic stage through the direct development of the actinula larva. The second is a short contribution by S. Lefevere on the occurrence and significance of esterolytic enzymes in the intestine, and of alkaline phosphatase in the nephridia, of *Nereis diversicolor*. The next two papers are devoted to invertebrates of tropical Africa—the first a study of the Vertiginidae and Valloniidae of Central Africa by W. Adam, and the second a review by A. Capart of the types of the species of freshwater crabs of the family Potamonidae.

A paper by J. Cooreman gives the results of a two-year ecological study on the hydracarine fauna of a stream in a peat bog in the Hautes Faynes, Belgium; H. Synave discusses African Hemiptera Homoptera and describes a new species of the genus *Cordia*; and A. Collart describes two new species of *Microsania* (Diptera: Platypezidae) from Belgium. G. Fagel makes his twenty-seventh contribution to the study of the staphylinid beetles and describes numerous new species of the genus *Paederus* from the Belgian Congo; and A. Janssens gives his seventeenth contribution to the study of lamellicorn beetles with a description of a new species of *Onthophagus* from the Upemba National Park, to which is added a discussion on the evolution and convergence of certain scarabid beetles.

The first paper on the vertebrates is a revision of the Amphisbaenidae of the Belgian Congo by G. F. de Witte, based on the material in the Musée royal du Congo Belge and the Institut des Parcs nationaux du Congo Belge, many of the specimens having been collected by the author himself. R. Verheyen discusses the migrations of the European population of the ruff and shows that on the northward migration the majority of European ruffs cross the Mediterranean to northern Italy, from which they disperse to their nesting-grounds all over Europe; individual ruffs constantly return to the place of their birth. S. Frechkop discusses the genetic relationships between the anthropoid apes and the other primates. He concludes that it is more satisfactory to substitute for the unconvincing idea of migrations of primates from Africa to the Far East and vice versa, or the unproved idea of hologenesis, the theory of the manifestation of analogous potentialities of evolution (of the same genes) in parallel in Asia and Africa under similar ecological conditions. F. Twiesselmann contributes a long paper on the basic principles of anthropology and stresses the importance of anthropometry in the study of races and populations. P. Jolivet returns to the invertebrates with a study of the evolution of the phytophagous habit in the chrysomelid beetles, and suggests that the passage from primitive polyphagous habits through oligophagy to monophagy has been brought about by a series of restrictive mutations, so that the most striking adaptation produced by selection is the instinctive behaviour that leads to the choice of the food plant.

E. Kesteloot discusses the evolution of the flora and fauna of Flanders and shows that climatic changes have caused, and are causing, Atlantic and Mediterranean forms to replace boreal types, and that the coastal belt, which has been greatly changed through the past two centuries by afforestation, is the region through which adventitious species, both of plants and animals, generally become introduced. L. Van Meel reports the existence of supersaturations of oxygen in certain natural waters, and suggests that it is produced by the action of ultra-violet light which decomposes the water with the formation of hydrogen peroxide or free oxygen. This radiation is injurious to organisms containing chlorophyll, and the author correlates oxygen supersaturation with scarcity of phytoplankton.

The final article, by L. Debot, gives an account of the achievements of the Service Éducatif of the Institut royal des Sciences naturelles de Belgique during the twenty years since it was set up by Prof. Van Straelen.

The first volume contains an excellent photographic portrait of Prof. Van Straelen, and instructive contrasting aerial views of the Institut as it is to-day and as it was twenty-eight years ago.

These volumes are a fitting tribute to the work of Prof. Van Straelen during more than a quarter of a century, and the wide field covered by the numerous articles mentioned above testifies to his many interests and the multifarious branches of the natural sciences which he has taken so active a part in promoting in Belgium. The Institut royal des Sciences naturelles, enlarged out of all recognition since Prof. Van Straelen became its director, is a lasting memorial to his work, and these jubilee volumes are the expression of gratitude and esteem from every branch of his staff.

L. Harrison Matthews

NUCLEAR SPECTROSCOPY

Beta- and Gamma-Ray Spectroscopy

Edited by Prof. Kai Siegbahn. (Series in Physics.) Pp. xxiii+959. (Amsterdam: North-Holland Publishing Company, 1955.) 76 guilders; 152*s.*; 20 dollars.

THE new experimental techniques developed during and since the War have led to a revolution in nuclear spectroscopy. New designs of beta spectrometers give a high precision in energy measurement (one part in two thousand), combined with excellent resolution. Scintillators provide highly efficient counting devices for gamma rays, so that very weak radiations can be detected. High-speed coincidence circuits facilitate the unravelling of complex spectra and the measurements of lifetimes of nuclear states down to 10^{-10} sec. At the same time the number of available artificial radioactive species has increased because of the development of nuclear piles, and the larger number of particle accelerators in use.

On the theoretical side, the nuclear shell model and the collective model have provided a basis for the interpretation and classification of complex experimental data. Precise computations of internal conversion coefficients, hand-in-hand with their precise measurement, now make it possible to decide the multipolarity of gamma rays, and so the changes of spin and parity in the nuclear transitions

involved. A better understanding of the theory of multipole transitions in nuclei also enables one to obtain this information from the lifetimes of nuclear states. The theory of beta decay has been slower in developing and the full nature of the fundamental interaction is still not known, though it is now fairly certain that a linear combination of the tensor and scalar invariant interactions, with possibly some pseudoscalar, is the correct one.

The subject is therefore ripe for a comprehensive survey. This is what is attempted in the present volume. There are twenty-six chapters by different authors, dealing with different aspects of beta and gamma ray spectroscopy. Some chapters are further divided into short contributions on related topics by different contributors.

It is inevitable in such a project that there should be some overlap, and it is to the editor's credit that he has kept it down so well, while at the same time covering so much ground. In addition to the techniques of nuclear spectroscopy, which are very fully and usefully covered, the theory of allowed and forbidden transitions, of multipole radiation and internal conversion, nuclear models, the technique and theory of angular correlations, the empirical classification of beta transitions and nuclear isomers, illustrative studies of particular disintegration schemes, there are also sections on such topics as double beta decay, positronium, the decay of positrons and the double Compton effect. The study of gamma radiation from charged-particle reactions and from neutron capture, both of which give information about the more highly excited states of nuclei than is usually obtainable from beta radioactivity, are also each given a chapter.

The book is well printed and well illustrated (though some of the figures which appear as plates are collected together away from their proper pages without any guide in the text as to their whereabouts). It will be indispensable to any laboratory where any form of nuclear physics is practised, and indeed in any university library, though its price is likely to restrict its being bought by many individuals.

A particularly valuable feature is in the numerical tables at the end. They contain the numerical information needed for almost any evaluation of experimental data: internal conversion coefficients, Fermi functions, X-ray critical absorption energies, angular correlations, conversion tables from $B\rho$ to energy in keV., γ-ray absorption coefficients.

The author, and subject, indexes seem to be full and accurate.

M. H. L. Pryce

A CHAUCER HOLOGRAPH?

The Equatorie of the Planetis

Edited from Peterhouse MS. 75, I, by Dr. Derek J. Price, with a linguistic analysis by R. M. Wilson. Pp. xvi+214+14 plates. (Cambridge: At the University Press, 1955.) 52*s.* 6*d.* net.

In the course of collecting data for a general history of scientific instruments Dr. Price came across a manuscript in the library of Peterhouse, Cambridge, which was written in Middle English, was on an astronomical subject, and in which the date 1392 occurred frequently. He thought that it might be the missing part of Chaucer's "Treatise on the Astrolabe" written in 1391, which was at the time the only instrument tract known to him that was written in Middle English.

The manuscript has been in the library of Peterhouse since 1542 and has been attributed since some date prior to 1542 to Simon Bredon. But he could not have written it since he was dead by 1372, when his will was proved.

The manuscript contains directions for the construction and use of the equatorium (or equatorie, as the manuscript anglicizes the term). This instrument was a planetary calculator, based on the Ptolemaic theory of the motions of the planets, designed to avoid the bulk of the computations involved in deriving planetary positions from the Alfonsine Tables in use at the time. The equatorium, in fact, provides a geometrical construction based on the epicycles and equants of the Ptolemaic theory; to derive the position of a planet at a given time, the mean motus and mean argument are taken from the tables and all else is performed by setting the instrument.

The equatorium, of which there were several different types, was widely used in medieval times; but, in contrast to the astrolabe, few have survived. Only two incomplete examples have been traced; one, in the possession of Merton College, has many features in common with the instrument described in this text. Dr. Price attributes the survival of large numbers of astrolabes to the fact that they could readily be embellished with the art of the metal worker: the equatorium was much more complicated and could not readily escape a purely utilitarian appearance and hence had much less chance of being preserved.

The special interest of this manuscript is that it may be an original work by Chaucer, written by his own hand. The linguistic analysis by Mr. R. M. Wilson shows that it contains many words not recorded elsewhere in Middle English except in the works of Chaucer. There is a reference in it to the "tretis of the astrelabie". The whole of it was written by the same hand, which in its informality is in strong contrast to the formal scribal writing of the period. There are numerous corrections and additions by the same hand. The manuscript may be a translation and adaptation from a Latin original; if so, the original has not survived. There is little doubt that it was written about 1392, probably shortly after the "Treatise on the Astrolabe".

The authenticity of the manuscript as a Chaucer holograph could be established if it could be compared with a genuine specimen of Chaucer's handwriting. Unfortunately there are no certain autograph Chaucer documents known. A document was discovered some few years ago among the Exchequer Bills in the Public Record Office which it is thought may be in Chaucer's hand; it relates to the appointment of a deputy for him, as Comptroller of the Wool Quay in the Port of London, during his absence. On one of the pages of the Equatorie treatise is a note which mentions Chaucer by name; the writing of Chaucer in this note is strikingly similar to the Chaucer in the above-mentioned document, as may be seen from the enlargements given in this volume. Though conclusive proof is lacking that the treatise is a Chaucer holograph, the evidence in its favour is very strong.

In this volume the text is reproduced in facsimile with transliteration opposite, and a translation into modern English is given. The critical commentary includes an analysis of the contents of the manuscript

and of the tables, an account of the Ptolemaic planetary system and of the theory of the equatorium, a history of the equatorium, the linguistic analysis by Mr. R. M. Wilson, and a detailed discussion of the possible ascription to Chaucer. The production of the volume is of the high standard which is characteristic of the Cambridge University Press.

H. Spencer Jones

THE PROBLEM OF RACE

The Colour Problem

A Study of Racial Relations. By Anthony H. Richmond. (Pelican Book No. A. 328.) Pp. 371. (Harmondsworth, Mddx.: Penguin Books, Ltd., 1955.) 3*s.* 6*d.*

An Essay on Racial Tension

By Philip Mason. Pp. ix+149. (London and New York: Royal Institute of International Affairs, 1954.) 8*s.* 6*d.* net.

THE interest which has recently manifested itself in Great Britain in the problem of race shows signs of becoming steadily more widespread. This is evidenced by the publication of Mr. Richmond's book in the Pelican Series, which will no doubt sell in tens of thousands. The fact that it is also becoming deeper is also to be deduced from Mr. Mason's appointment as director of studies on race relations by the Royal Institute of International Affairs. The books that it has published, however, appear to show that there is not yet a great deal that can be said to an English audience on which remedial action can be based. The present endeavour is to present the facts and remove misconceptions.

Mr. Richmond gives us a comprehensive and scholarly survey, primarily from the historical and political point of view, of the patterns of race relations in South Africa, British Colonial Africa and the British West Indies, and in Great Britain itself, which brings together in a very useful form a large mass of information which would otherwise not be available to the general reader. It is presented cogently, and leads to the conclusion that "so-called 'white civilization' will only be preserved by sharing its fruits with non-Europeans, not by depriving them of its benefits". Mr. Mason's "Essay", on the other hand, is primarily directed towards summarizing the theories and conclusions both of politicians and of scientists, and he seeks to test the validity of their ideas by applying them to the situation in South Africa. The result might have been a work which could be regarded as complementary to Mr. Richmond's; unfortunately, however, Mr. Mason gives an impression that he is 'writing down' to a rather ignorant reader, lacking anything more than a conventional grammar school education, as a target to aim at. Even so, it would have been well if he had demonstrated more clearly that the subject of racial tension is a difficult one to grasp. The recent publications of Unesco show that a great deal of disagreement still exists among physical anthropologists and geneticists on the subject of race, and any glossing-over of this fact necessarily detracts from the value of a work in which the point of view of the scientist is discussed. The concept of race, as applied to human societies, is indeed a very shaky one, and this needs to be made plain.

Mr. Richmond and Mr. Mason agree on one important point, which is that the difficulties that have arisen when representatives of different 'races' have been brought into close association with each other can only be dealt with by a slow process of amelioration or reform. "The elimination of prejudice," writes Mr. Richmond, "will almost certainly be a long and gradual process." This conclusion is echoed in Mr. Mason's statement that "there is no quick or easy way to knowledge, and it is only by knowledge, and by knowledge based on study, that any firm convictions can be reached. In spite of the urgency, plans for study must be made on the basis that thoroughness must come first and speed second". These are the final thoughts of a sociologist and a scholar, who share a concern for the welfare of mankind and an awareness of the rapidity with which events are now moving. But it must be asked whether these are really the last words that can be said on the subject. Social scientists, legislators, and administrators in the United States would probably reply that, given a firm grasp of the objectives that have to be attained, and a determination to attain a substantial proportion of them without delay, much can be achieved in the immediate future. The success with which the colour bar has been broken down in recent years in the District of Columbia, and the extent to which the educational system of the country is being successfully reorganized on an inter-racial basis as the result of the Supreme Court decision, provide evidence to justify this conclusion. There is much food for thought in this for the British citizen.

T. S. Simey

ELECTRIC AND MAGNETIC PROPERTIES OF THIN FILMS

Conductibilitié Électrique des Lames Métalliques Minces

Par Prof. A. Blanc-Lapierre et Prof. M. Perrot. (Mémorial des Sciences Physiques—Fascicule 57.) Pp. iii+96. (Paris: Gauthier-Villars, 1954.) 1100 francs.

Propriétés Magnétiques des Lames Métalliques Minces

Par Prof. M. A. Colombani. (Mémorial des Sciences Physiques—Fascicule 58.) Pp. iii+78. (Paris: Gauthier-Villars, 1954.) 1000 francs.

THESE two small monographs may be regarded in a way as similar to the reviews published in England in *Progress in Physics*, except that they pernaps assume less knowledge on the reader's part.

Vol. 57 on the electrical properties of very thin metallic films is very well written, and the authors take great pains in discussing the advantages and disadvantages of known methods of preparing metal films for precise measurement. All interested in the way in which the electrical conductivity of thin films evolves following their preparation, the effects of temperature upon the conductivity, the variation of conductivity with field strength which is in effect the breakdown of Ohm's law, the physical structure of such films and the theoretical interpretation of their behaviour, will find much to interest them in this little volume.

Vol. 58, on the magnetic properties of very thin films, is also interesting, though one feels that the diagrams might have been produced with the same care as in the preceding monograph. Naturally,

one finds here that much more attention is paid to films deposited electrolytically, and it is interesting to note how well Maurain laid the bases of this part of the subject some forty years ago. Since it is found that in the case of very thin films of ferromagnetic metal the hysteresis cycles are rectangular, it is natural to interpret their behaviour in terms of single domain particles, and the relevant theory is adequately discussed in the final pages of the book. As sources for valuable references both little volumes will be found useful. L. F. BATES

CATALYSIS

Catalysis

Edited by Paul H. Emmett. Vol. 1: Fundamental Principles (Part 1). Pp. vi+394. 80*s*. net. Vol. 2: Fundamental Principles (Part 2). Pp. vi+473. 96*s*. net.
(New York: Reinhold Publishing Corporation; London: Chapman and Hall, Ltd., 1954 and 1955.)

Advances in Catalysis and Related Subjects

Edited by W. G. Frankenburg, V. I. Komarewsky and E. K. Rideal. Vol. 6. Pp. xiii+467. (New York: Academic Press, Inc.; London: Academic Books, Ltd., 1954.) 10.50 dollars.

THE present active state of catalysis makes this a very suitable time for the appearance of a new and comprehensive treatise on this subject, if only as a supplement to somewhat older works such as the "Handbuch der Katalyse" and as a parallel effort to the current series of monographs included in "Advances in Catalysis".

The general pattern which has been followed is the now usual and necessary one involving the assembly of a number of articles by individual contributors under a general editor or editorial panel. Vol. 1 of "Catalysis" contains eight chapters, most of which give a clearly written account of the present state of knowledge in the field treated. Of these, the chapter by Dr. Emmett on the measurement of the surface area of solid catalysts will be especially welcomed for its critical treatment of surface area determinations by the Brunauer–Emmett–Teller and similar methods. An article by H. E. Ries on physical adsorption may be regarded as an introduction to this chapter. The subsequent series of three sections by K. J. Laidler, on chemisorption, on kinetic laws in surface catalysis, and on the absolute rates of surface reactions, are all admirable in their treatment and clarity. This applies also to the section on magnetism and catalysis by P. W. Selwood; indeed, in view of the importance of paramagnetic characteristics in the theory of the catalytic activity of metals of the eighth group, it would have been welcome if this chapter had been longer and if it had included recent work on the observed parallelism between the suppression both of the catalytic activity and of the paramagnetic character of these metals by alloying them with diamagnetic metals.

The volume contains two chapters of a type somewhat different from the above strictly fundamental treatment of catalytic principles, in that, in these, catalysis is dealt with mainly from an industrial point of view. Thus, in the section by W. B. Innes on catalyst carriers, promoters, accelerators, poisons and inhibitors, a good general description of porous and other carriers is followed by the tabular presentation of long lists of uncriticized and undiscussed references to patent literature, with the corresponding references to *Chemical Abstracts*. While this extensive use of patent literature might perhaps be justified on grounds of the importance of the industrial uses of catalysis, it is difficult to obtain precise and reliable information from such sources; moreover, in tables of recent literature, some inaccuracies have been noticed, presumably on account of failure to check back to the original papers. The following chapter, on catalyst preparation, by F. G. Ciapetta and C. J. Plank, also has an industrial trend but contains adequate references to journal literature. A minor criticism might perhaps be made with respect to the brevity of the descriptions of the preparation of ordinary catalysts in the text of the article, and to the confinement of detailed preparative directions to a few specialized industrial catalysts.

Vol. 2 is, on the whole, more consistently fundamental in character than Vol. 1. The chapter on theories of heterogeneous catalysis, by H. Eyring and others, contains a comprehensive account of modern views of chemisorptive catalysis, including the thermodynamics and general energetics of chemisorption, and a discussion both of the concept of activated adsorption complexes and of the structure of catalytic surfaces. It must, of course, be borne in mind that, in addition to the chemisorptive type, many reactions are known, both in heterogeneous and in homogeneous catalysis, which obviously operate by cyclic valency changes in the catalyst. The above treatment is amplified by two chapters, both by H. M. Hulburt, on the nature of catalytic surfaces and on that of catalyst–substrate complexes on such surfaces. All these chapters are well planned and, taken together, give a clear review of the theoretical foundations of heterogeneous catalysis. The section on reaction rates and selectivity in catalyst pores, by A. Wheeler, is welcome and timely by reason of the complications which arise by the retention of products within porous catalysts, in that this retention retards the free access of further reactants and, in some cases, modifies the nature of the product.

The structure of some of the sections, particularly in Vol. 1, does not tend to make unnecessary the consultation of original literature. The complete work should, however, become a standard one for inclusion as a matter of course in chemical libraries; and it should be attractive, perhaps specially to industrial chemists, both as a source of technical information and as a synopsis of the theoretical background of catalysis.

"Advances in Catalysis" continues to perform a most useful function by providing, in a convenient form, authoritative and critical summaries of current catalytic theory and practice.

The present volume, which is prefaced by an obituary notice and appreciation of Alwin Mittasch, contains seven further reviews. In the first of these, J. T. Davies discusses catalysis and reaction kinetics at liquid interfaces, with particular reference to the fundamental work of Sir Eric Rideal and others on such reactions. Systems of this nature form an intermediate class between homogeneous catalysis and the far more widely studied case of heterogeneous catalysis at a solid surface; and this review is specially welcome as an emphasis on the increasing importance and interest of reactions, both catalytic and non-catalytic, in the surface film between two liquids. The subsequent monograph, by T. Kwan, of

Hokkaido University, Japan, deals with chemisorption and catalysis. In spite of the greater familiarity of this ground, this section makes stimulating reading and contains evidence of considerable original thought.

A short chapter, by W. P. Dunworth, summarizes recent work on the use of polyvinyl alcohol as a protective colloid for stabilizing finely divided palladium catalysts. This employment of colloids as anticoagulants dates back to the work of Paal and others at the beginning of the present century; but, unlike the colloids then used (for example, protein degradation products or even natural gums), the newer synthetic plastics now available can be dispersed in non-aqueous liquids. Highly dispersed palladium or similar catalysts, when stabilized in this way, retain their large specific surface and have accordingly a high and stable activity and a low sensitivity to poisoning. P. B. Weisz and C. D. Prater discuss the derivation of catalytic activity constants, of activation energies, and of reaction orders, from measurements carried out with porous catalysts and with special reference to diffusion factors introduced by this porosity. This also is a suggestive article, and includes an attempt to separate the effect of diffusion factors from factors inherent in the true catalytic activity. In a further section, some aspects of acidic and basic catalysis in homogeneous systems are reviewed briefly by M. Kilpatrick. Finally, two modern applications of catalysis in large-scale industry are treated at considerable length in sections on catalytic cracking and on the commercial isomerization of hydrocarbons, written respectively by E. V. Shankland and B. L. Evering.

The volume, taken as a whole, lives up to the high standard of its predecessors. It is, of course, a mixed collection; but the general presentation is good. The editors are to be congratulated on assembling, in the course of the successive volumes, such a representative series of topical reviews, written by authors of many nationalities; but the field of catalysis is very wide and rapidly expanding. The transition of research in catalysis, from a purely empirical approach up to rational planning on the basis of theory and of known regularities, is already well marked. Thus the statement in the obituary notice of Alwin Mittasch that, some fifty years ago, no less than 20,000 different specimens were tested for their activity as catalysts for the synthesis of ammonia, might be contrasted with the certainly far less laborious way in which catalysts for a given reaction can in most cases be selected to-day. This new volume of "Advances in Catalysis" will need no special recommendation on the part of the reviewer.

E. B. Maxted

MAN AND ANIMALS

Animals, Men and Myths

A History of the Influence of Animals on Civilization and Culture. By Morus (Richard Lewinsohn). Pp. 374+31 plates. (London: Victor Gollancz, Ltd., 1954.) 21*s*. net.

THIS is an interesting and readable work which embraces a very extensive field. The author, who uses the pen-name of "Morus", is Dr. Richard Lewinsohn, the head of the Institute of Technical Research in Rio de Janeiro. His book, originally written in German, and now translated into English, is divided into six parts: (1) Prehistory. The primitive world. The origin and distribution of living forms. The ape-like ancestors of man. (2) Domestication and exploitation of animals. The amenities of the cave dwellers. Animal fables. (3) The age of discovery and ideas. Inter-relations of man and animals. Discovery of new worlds. The part played by animals in providing the materials for man's clothing. Fabulous animals. (4) The speculative age. Scientific organization of the animal kingdom. Probability of evolution. The new southern continent. Experimental zoology. (5) The age of machines. The Darwinian revolution. Parasitology. Animal intelligence. (6) The age of modern chemistry. Genetics. Animal economy and future.

It is obvious that to cover accurately so catholic a syllabus demands from its author encyclopædic knowledge, and from its translator the command of an attractive literary style. We congratulate the producers of this book on the possession of these qualifications, which have enabled them to compile a work not, it is true, free from mortal error, but sufficiently attractive in presentation to guarantee that the intelligent reader will be reluctant to put it down. The underlying theme is the mutual dependence of man and animal, which is perhaps overstressed in the statement that "the future existence or non-existence of animals is in fact more and more dependent upon the will of man". The illustrations are numerous and have been well selected, with a few exceptions such as the poor figure on p. 328 after Lewis Carroll, who, great as were his creative gifts as a writer, was no substitute for John Tenniel as an illustrator. In the translation we must deplore occasional lapses into the figurative language of the boxing ring. For example, the account of the famous meeting of the British Association at Oxford in 1860 is grossly misleading in every particular. The bishop in his manner was eminently suave and restrained. He did not "yell" at Huxley, nor did he "grab a club and smash away". In reply, Huxley did not "land a smasher on the bishop's chin", nor did he describe his attack as "sticking his nose into scientific questions". The present reviewer has not seen the German edition, and hence can only hope that such defects of the English version have no place in the German original.

Among the points requiring attention in a new edition are the following: p. 153, "Avicenna of Cordoba" [*sic*] died in 1037, and was not "one of the most famous doctors of the twelfth century"; p. 189, the term spermatozoa was not introduced by Leeuwenhoek, but by von Baer in 1827; p. 220, Sir Joseph Banks neither discovered nor named the duck-mole. It was discovered by David Collins in 1797, and the first printed description was by G. Shaw in 1799. Collins did not publish his own account until 1802. The Governor of Australia sent two skins of the duck-mole to Banks in 1800, one of which was passed on to Blumenbach, who described and named the species in that year; p. 233, Spallanzani's skilful and delicate regeneration experiments are not aptly described in such language as "he proceeded to slice up a salamander" [? newt] and "tore off" its legs, feet and jaws. Spallanzani's name is incorrectly spelt both in the text and index; p. 272, Darwin did not retire to the Isle of Wight for fourteen months to write the "Origin of Species", nor did he ever write a "thick book on fossil mollusca".

Despite its occasional shortcomings, the book is well worthy of the attention of the uninitiated, who will derive from it much delightful instruction.

F. J. COLE

NATURAL CONTROL OF ANIMAL POPULATIONS

The Distribution and Abundance of Animals

By H. G. Andrewartha and L. C. Birch. Pp. xv+782. (Chicago: University of Chicago Press; London: Cambridge University Press, 1954.) 112*s*. 6*d*. net.

THIS large monograph begins by rejecting the relevance of any study of whole animal communities or ecosystems to the understanding of distribution and numbers of animals. It then proceeds to take a firm stand against the general use of mathematical models and, in particular, of ideas connected with biotic relationships and density-dependent processes between populations, as explanations of events in Nature. The authors believe that the regulation of numbers does not exist in the sense hitherto examined in theoretical and laboratory population ecology. They believe that most fluctuations consist of the natural recovery of populations each time after minor or major catastrophes, or at any rate changes, which have reduced them, and that these are usually brought about by weather; that is, the system of control of numbers in Nature contains only one compensatory process—the innate capacity for increase, pitted against the chaos of outer Nature. This summary of their views is, of course, necessarily a simplification of a tome that contains an extremely large amount of hard intellectual work, valuable case-histories from a number of laboratory and field studies by various people, and a genuine attempt to reduce to logical order the very diffuse field of population ecology (though even here, it is chiefly terrestrial ecology and, within that, the ecology of insects that has provided the main material).

After about fifty pages of preliminary statements, and two chapters on diapause and on dispersal, taking the reader to p. 125, there are two hundred pages about the effects of temperature, moisture and light, which are interesting in their own right as source material, necessary to the later theory, but not in themselves conclusive as regards the latter. This central section, "Analysis of Environment", continues with about two hundred pages more about biotic relationships such as competition within and between species, predator–prey relationships, food, and so forth. Most of this section about population relationships is written in a highly critical, often polemical, manner, and it forms a sustained and oddly contemptuous attack on what the authors regard as the dogma of the regulation of numbers through varying density-dependent processes based proximately upon biotic links. It does not seem likely that the reader will be entirely impressed by the implication that nearly all previous research in the subject is irrelevant to the interpretation of field population changes, or that the study of simplified mathematical models and their testing in the laboratory is misleading. There is certainly a real dilemma, recognized by all sensible ecologists, between studying the complexity of naturally interspersed populations with their ecological connexions, subject to variable outer conditions of climate and vegetation (and often the actions of man as a superpower), and the delimitation for study under controlled conditions of parts of this system at a level of precision that will satisfy scientific criteria.

Apart from the last fifty pages, which deal with some genetic matters in ecology, the rest of the book is an attempt to provide a new theory of the control of population density, illustrated by a few examples from the field. The latter include some of the authors' own research upon insects in South Australia. The facts adduced are most interesting, and should be studied dispassionately by ecologists. The trouble one has is that there are as yet practically no field measurements of populations of anything like the refinement possible in the laboratory. For all practical purposes at the present time, we know no more about the behaviour in the field of the innate capacity for increase than Malthus did. The notion of innate capacity for increase, with its stable age distribution, is really quite as highly theoretical and abstract a tool for study as the equation for the logistic curve, or Volterra's predator–prey theories. Apart from the human material originally used, it is so far based entirely upon laboratory work with populations living in supposedly defined conditions. The authors have, indeed, substituted for what they consider a dogma, another just as theoretical and with rather less common sense behind it. In spite of the air of certainty that pervades this large book, there are striking inconsistencies like this. These may not appear obvious at first reading. The style is lucid and the learning great, but the thoughts behind are not always illuminated. It is not clear why so large a part of the book deals with density-dependent processes studied in the laboratory, and yet rejects their application to field events. There are some strange remarks on important topics: for example, that something cannot be a general theory unless it describes a substantial body of empirical facts; that "one would not expect to come across a limiting density in any finite number of observations"; and that only individuals, not populations, can be said to have an environment.

There is, however, so much that is useful and provocative to thought, and it is so sincerely tackled and expounded, that one can forgive the authors a certain amount of word-splitting and special pleading. It is less easy to overlook their evident refusal to understand the way in which scientific thought has to advance in a complex field, by the proper use of abstractions and inductive reasoning. Perhaps it is a good moment for ecologists to try to assess how far our existing knowledge can take us in deciding the relative parts played in the regulation of numbers by self-limitation (though intra-specific competition), other biotic relations, and influences like varying weather, refuges and the like. But I cannot believe that the natural communities of the world will be found to have evolved solely "by guess and by chaos", or that the inter-relations of animal populations have not introduced a gradual order into the structure of animal communities.

The book is well produced, though it could have been half as large and had ten times as many readers; there are, however, some misprints in mathematical formulæ. All research ecologists are recommended to read it in order to try to decide whether, as the dust-cover announces, it provides "a new, satisfying and workman-like theory".

C. S. ELTON

MODERN TRENDS IN CHEMOTHERAPY

By Sir CHARLES HARINGTON, F.R.S.
Director, National Institute for Medical Research

CHEMOTHERAPY, or the treatment of infective diseases by means of drugs, as an empirical method of treatment is no new thing. Two examples that are commonly quoted to illustrate the long history of the chemotherapeutic method are the use of mercury for treatment of syphilis and of quinine for malaria. Chemotherapy as we now understand it, however, is a very different matter; it is a subject for scientific research, requiring for its development the application of many different methods of investigation, and the ever-increasing range of its applications has transformed the treatment of protozoal and bacterial infections in man and animals.

The realization of the potentialities of chemotherapy and the systematic development of the subject have come about during the past fifty years, and they are almost entirely based on the work of Paul Ehrlich, the centenary of whose birth was celebrated in 1954. The present time seems appropriate for a brief review of chemotherapy as it stands to-day and of its probable lines of future progress.

Ehrlich himself had a perfectly clear idea of what he was aiming at in the successful chemotherapeutic drug. He sought compounds which were specifically bound by, and toxic to, infecting organisms, while being relatively harmless to the host; and he was led to believe in the existence of such compounds by his observations on the specific affinity of dyes for certain cells and tissues; his first successful experiments, indeed, were in the treatment of trypanosomal infections of animals with dyestuffs. When these experiments turned out to be without practical significance for the treatment of human infections, Ehrlich turned his attention to other types of chemical compound, and, building on the earlier observation of the trypanocidal action of the organic arsenical compound atoxyl, he was led to his great discovery of the efficacy of arsenical compounds in the treatment of syphilis.

This discovery, with its striking demonstration of the possibility of successful specific treatment of an infective disease by a synthetic drug, was the real starting point of the great effort that has been thrown into chemotherapeutic research. This effort, as was natural, was first mainly concentrated in Germany, where the highly developed chemical industry was inspired by Ehrlich's early successes to devote great resources to the search for new drugs. For a long time there seemed to be little prospect of effective chemotherapy outside the field of protozoal infections; even with this limitation, however, the subject was clearly one of great interest to a country like Great Britain, with large Colonial territories in tropical and sub-tropical regions, and in fact the Medical Research Council has from the outset done all in its power to foster chemotherapeutic research. In recent years this type of research has also become a major interest in the United States, and from that country have come some of the most important discoveries.

Unfortunately, the impetus provided by Ehrlich's discovery, although stimulating a great amount of work, provided no theoretical basis for the systematic development of the subject, and the ensuing attempts to find new drugs remained for long almost entirely empirical. Certain clues were indeed available: one example of successful chemotherapy with a long history behind it, namely, the use of quinine in the treatment of malaria, has already been mentioned; this clue was not, however, directly followed, and the line that was pursued, chiefly by the German chemical industry, started rather from the dyestuffs among which Ehrlich had observed chemotherapeutic activity in experimental infections. Knowledge of the relationship between chemical structure and biological action was, however, not sufficiently advanced to make this type of approach truly effective, and although valuable drugs, such as the antimalarials plasmoquin and mepacrine, resulted from it, the yield of useful products was discouragingly small in proportion to the expenditure of time and effort.

Even in the early days of chemotherapeutic research, the evidence was available that real advances could only be made where there was the closest collaboration between chemists and biologists; the success of the German effort depended not only on the great potential of the German chemical industry but also on the importance attached by the pharmaceutical section of this industry to strong biological research departments, an example followed in recent years with outstanding success by Imperial Chemical Industries, Ltd., in Britain and by several industrial organizations in the United States. Moreover, in Britain we may recall the close association of Prof. C. H. Browning, of Glasgow, with the late Prof. J. B. Cohen, of Leeds, in the development of valuable antiseptics, and with the late Sir Gilbert Morgan and Dr. L. P. Walls in producing the important phenanthridinium series of trypanocidal drugs; we may also recall the long-continued work of Dr. Harold King on chemical constitution and trypanocidal action, which was carried out by him within the biological environment of the National Institute for Medical Research, and the collaboration of Dr. King with the late Prof. Warrington Yorke, of Liverpool, which led to the diamidine drugs, useful in the treatment of kala-azar. These successes may be contrasted with what can now be seen to have been missed opportunities in researches undertaken by chemists without any definite provision for biological collaboration, or by biologists who were unable to bring their observations to the notice of chemists who might have helped to develop them. Many chemotherapeutic researches have been initiated in departments of organic chemistry, and much work of high quality has been put into these projects with disappointingly little in the way of practically useful results. On the other hand, already in 1898, in a letter to Carl Weigert, Ehrlich put forward a suggestion which came very close to the Woods–Fildes theory that will be referred to later; but biochemists were not available to bring his ideas to fruition. Again, penicillin might have been brought into use at a much earlier date had the potentialities of Sir Alexander Fleming's original discovery been more fully realized by chemists and biochemists.

In retrospect, it seems that the necessity for close collaboration between chemists and biologists in research on chemotherapy should have been so obvious as to be universally recognized from the

outset. That it was not so recognized was due in part to the absence of underlying theory, and to the fact that discoveries made by chance or on the basis of slender and even sometimes very dubious reasoning seemed to indicate that the prospects of developing any comprehensive theory were remote. In these circumstances, the large-scale, semi-empirical method of screening new synthetic organic compounds appeared to offer the best chance of success.

Two principal factors have contributed to the very different outlook on chemotherapeutic research that prevails to-day. One is the great broadening of the field of application of chemotherapy that has resulted, first from Domagk's discovery of the antistreptococcal action of prontosil and the consequent development of the sulphanilamide drugs, and secondly from the discovery of the antibiotics. The other is the increase in knowledge of the biochemistry of micro-organisms and the realization of the importance of this subject for the understanding of the mode of action of drugs on pathogenic bacteria.

The latter advance is due in large measure to the work of the Medical Research Council's former Bacterial Chemistry Research Unit under the direction of Sir Paul Fildes. By patient investigations over many years, the workers in this Unit had acquired extensive knowledge of the factors important in bacterial growth and metabolism. It was this work that paved the way for the discovery by D. D. Woods that the sulphanilamide derivatives exercised their anti-bacterial effect by interference with an essential bacterial metabolite closely similar in chemical constitution to sulphanilamide itself, namely, *p*-aminobenzoic acid. On the basis of this observation, Sir Paul Fildes advanced what was, in fact, the first satisfying theory of chemotherapy; according to his hypothesis, an effective chemotherapeutic agent will be a substance that interferes with some essential metabolic reaction of the pathogenic micro-organism, and such a substance should be sought among compounds having close chemical similarity with those normally utilized by the organism in its metabolism. The Woods–Fildes theory is in reality a special application of the generalization that emerged from the much earlier work of Prof. J. H. Quastel and his colleagues, in which it was shown that enzymic reactions are inhibited by compounds that are chemically similar to the normal substrates; its development is thus an excellent example of the way in which an academic biochemical research can lead to an idea of practical importance in medicine.

It is only now that the full value of the Woods–Fildes theory is becoming apparent. Early attempts at its application were perhaps too naïve in conception, and although they produced numerous examples of interesting biochemical antagonisms, which clearly fell within the general framework of the theory, they did not lead to the discovery of practically useful drugs. In recent years, however, great advances have been made in knowledge of nutritional factors that are important or essential for the growth of micro-organisms; with the aid of this knowledge, and with more attention to the practical consideration that a successful chemotherapeutic agent must attack a metabolic reaction which is more vital to the infecting organism than to the host, we are beginning to see useful results. The most important development along these lines that has so far been made is the preparation of synthetic compounds designed to act as antagonists against folic acid; substances prepared with this object in view include not only pyrimethamine (one of the more successful antimalarial drugs) but also compounds that preferentially inhibit the growth of neoplastic tissue and thus offer some hope of chemotherapeutic attack on cancer.

In any effective consideration of chemotherapy, it is necessary to avoid too rigid adherence to the original conception of a chemotherapeutic agent as one that is damaging to the parasite and harmless to the host. The parasite is, after all, dependent on the metabolism of the host, and it is at the same time subject to attack by the host's defence mechanisms; one can therefore conceive of the existence of drugs which would arrest the course of an infection, not by direct action on the parasite, but by so altering the metabolism of the host that the parasitic environment becomes unfavourable, or by raising the normal defence mechanism of the host to a higher level of effectiveness.

Recent observations have shown that this conception is of more than theoretical interest. So far as the effect of host metabolism on a parasitic infection is concerned, two examples of the influence of metabolic changes in the host on the malaria parasite are now known. One is the relative resistance of animals on a pure milk diet to malarial infection; this was observed first by Prof. B. G. Maegraith and his colleagues at Liverpool, and further analysis of the observation by F. Hawking and J. D. Fulton in the National Institute for Medical Research has shown that the effect is almost certainly due to the deficiency in milk of certain dietary factors that are essential to the malaria parasite. The second example is given by the interesting suggestion by A. C. Allison that individuals with the sickle-cell trait, whose hæmoglobin is known to be abnormal, are relatively resistant to malarial infection. The first of these observations in particular suggests the possibility of drug treatment that would produce an alteration in metabolism, trivial for the host but disastrous for the parasite.

An example of a chemotherapeutic effect that is apparently exercised at least in part through the host's defence mechanisms is afforded by the action of the anti-filarial drug hetrazan. F. Hawking has shown that hetrazan has no deleterious effect on the microfilariæ in the test-tube, but that treatment with the drug renders them susceptible to very rapid destruction in the body. A more striking instance of chemotherapeutic action exercised through the host emerges from the recent work of P. D'Arcy Hart, R. J. W. Rees and J. W. Cornforth, at the National Institute for Medical Research, on the anti-tuberculous effect of certain high-molecular surface-active compounds (polyoxyethylene ethers of condensation products of substituted phenols with formaldehyde). Here again there is no suggestion of a directly damaging effect of the drug on the tubercle bacillus, but it has been shown by G. B. Mackaness, of the Sir William Dunn Department of Pathology at Oxford, that the monocytes isolated from animals treated with these surface-active agents have acquired the power of inhibiting the growth of tubercle bacilli—a power that is not possessed by monocytes isolated from untreated animals. In this instance, therefore, the natural cellular defences of the body have apparently been artificially reinforced, perhaps by a physical effect of the chemotherapeutic agent.

There is one important section of modern chemotherapeutic research that remains empirical and is

likely to continue to do so, namely, the search for new antibiotics; even in the field of antibiotic research, however, there is ample scope for basic biological investigation, particularly of the mode of action of the active compounds. Studies of this nature are important because of the aid they may give in directing synthetic chemical work along profitable lines; moreover, their help is likely to be valuable in the solution of the formidable problem raised by drug resistance, the development of which diminishes or destroys the efficacy of so many otherwise promising chemotherapeutic agents.

It is satisfactory to observe that considerable importance is being attached in Great Britain to the kind of biological research which, as is now clear, must form the basis of further advances in chemotherapy. Apart from researches that have already been mentioned, an example of this is the work of M. R. Pollock and his colleagues, at the National Institute for Medical Research, on adaptive enzyme formation in bacteria and on certain aspects of bacterial genetics; the results of such investigations may be confidently expected to throw light on the nature of the phenomena of drug resistance and drug dependence. A similar remark applies to the study of naturally occurring antagonists to antibiotics; streptomycin, for example, is a drug the use of which readily leads to the development of resistant organisms, and special interest is therefore attached to the quinoline-N-oxides recently shown by J. W. Lightbown and J. W. Cornforth to be streptomycin antagonists produced by *Pseudomonas aeruginosa*. The mode of action of antibiotics, especially in relation to bacterial metabolism, is one of the principal subjects of investigation in the Chemical Microbiology Research Unit of the Medical Research Council, while the work of Prof. H. A. Krebs and his colleagues at Sheffield has provided much of the sort of information about the tissue metabolism of higher animals that we have seen to be necessary for the consideration of chemotherapeutic problems in terms of the host as well as of the parasite.

What has been said may suffice to illustrate the way in which biological research has invigorated the whole subject of chemotherapy. From bacterial biochemistry there is emerging a satisfying theoretical background for the work. In the search for new synthetic chemotherapeutic drugs, we may expect to be guided in general by this theory and in detail by increasing knowledge of the biochemistry of bacterial metabolism, and by the results of study of the mode of action of naturally occurring antibiotics. The conception of chemotherapeutic agents has been widened to include drugs that may exercise their effects through the host rather than directly on the parasite, and the development of this aspect is being assisted by investigation of the cellular metabolism of higher animals. Chemotherapy has travelled a long way since the days when it amounted to little more than the empirical selection and biological test of organic compounds, whether already known or specially prepared for the purpose.

The emphasis that has been laid on the biological aspects of modern chemotherapeutic research must not be taken as in any way belittling the contribution that is needed from the organic chemist. The contribution is indeed greater than ever, because the chemist is needed now not merely to use his intuition in the supply of new compounds for test, but also as a full and equal scientific partner in a systematic research. The task of the biologist and biochemist is to provide the clues for new advance; the intellectual approach and special skill of the chemist are required in order that these clues should be followed up to advantage. No greater benefit could accrue to chemotherapy than a fuller realization by chemists that modern biological research has converted it into a subject that is worthy of their best endeavour, and is one from which they can expect to gain true intellectual satisfaction.

POLAR WANDERING

SOME GEOLOGICAL, DYNAMICAL AND PALAEOMAGNETIC ASPECTS

A COLLOQUIUM on the geological, dynamical and palæomagnetic aspects of polar wandering was held recently by the Department of Geodesy and Geophysics of the University of Cambridge, with Mr. B. C. Browne in the chair. Prof. J. W. Durham (University of California) spoke on the palæontological evidence on continental drift and polar wander, and said that continental drift, in the sense in which it is commonly used, involves a shifting of the continents both with respect to the poles and to one another; either or both of these movements could have occurred. Inasmuch as many proponents of continental drift consider it to have continued well into the Tertiary, it seems that this part of the theory can be adequately tested by comparing the Early Tertiary distribution of marine invertebrates with that now existing, as well as by comparison with fossil faunas the geographical relationships of which are well established.

From Thorson's data[1] on the larval life of marine invertebrates, it is apparent, said Prof. Durham, that a large proportion of a marine fauna in warm temperate or tropical seas could migrate across oceanic depths in an east–west direction for distances up to about nine hundred miles if there was a current similar to the present North Atlantic Gulf Stream; only a relatively few species could migrate across much greater distances. At the present time, out of 487 lamellibranchs and 1,104 gastropods living along the eastern American coast from Florida northwards, 81 lamellibranchs and 94 gastropods have also been recorded in Europe. Most of these species have an essentially continuous distribution from one area to the other; but 21 of the lamellibranchs and 49 of the gastropods seem to have a discontinuous distribution across the three thousand-odd miles of the North Atlantic. Likewise, along the warmer parts of the Pacific shores of the Americas there are 29 species of tropical molluscs and echinoids which also occur in Polynesia and the Indo-Pacific[2]. From the temperate Pacific coast of North America, more than sixty species have also been reported as occurring in Japan, and, of these, the specific identity of more than half has been confirmed by comparison of specimens. These and other instances indicate that a few molluscan species are very widespread in modern seas,

and that the occurrence of similar small numbers of identical species in widely separated fossil faunas cannot be adduced as proof of continental drift. Likewise, if areas are less than nine hundred miles apart and connected by seaways, it would seem that there should be approximately 50–60 per cent, or more, of the species in common in similar facies.

By comparison with living faunas, the climatic and bathymetric aspects of Cenozoic fossil faunas, provided they are of sufficient size, can be rather accurately determined. Examination of Early Tertiary marine faunas shows[3] that they were of tropical character in the State of Washington and near New York in North America, in the London and Paris basins, the Volga basin, north-western India, southern Japan, near the Cape in South Africa, in south-west Australia, southern New Zealand, and in Patagonia, as well as in the present-day tropics. When the warm temperate faunas that occur in the Aleutian Peninsula and Kamchatka are also considered, it is apparent, continued Prof. Durham, that the Early Tertiary North Pole must have been in approximately its present position and not in the North Pacific as the proponents of drift require. Eocene faunas of the Gulf Coast of North America have only two identical, and about nineteen analogous, species in common with European faunas of similar age. On a percentage basis, this similarity is no greater than that present in the living faunas. When contrasted with the 55 per cent of identical or closely related species occurring in the Indian lower-middle Oligocene and in north-west Italy—areas which at this time were connected by the well-documented Tethyan seaway and about three thousand miles apart—it is apparent that North America and Europe were as far apart then as now.

In the older part of the geological record, in so far as it is fossiliferous, it would seem that similar criteria should be used—that entire faunas and not single species or special groups of organisms should be considered in evaluating continental drift. For example, the common occurrence in Brazil and Africa, but not elsewhere, of the aquatic reptile *Mesosaurus* has long been one of the strong arguments for the close proximity of the two areas during the Late Palæozoic, for it was supposed to be restricted to a non-marine habitat; yet it has recently[4] been shown to be associated with marine organisms in Brazil.

The only marine fauna of the older Mesozoic and Palæozoic of Africa and South America which is adequately known and studied[4] is that of the Devonian Period. At that time the South African and Paraná basins had 46 species in common out of 120 species recorded from the Paraná basin and 175 in the South African basin. This is a greater number than were common to the Paraná and Bolivian basins, which were contiguous but had a different lithological facies. The similarity between the South African and Paraná basins (38 per cent) is not enough for the two areas to have been nine hundred or less miles apart as they are supposed to have been by the adherents of drift. The similarities are more probably to be explained by similar facies and the presence of an Atlantic island 'stepping stone' such as the Cretaceous Mid-Pacific Mountains[5] of the Pacific, the tops of which are now 5,000 ft. below sea-level and surrounded by oceanic depths, yet were once at sea-level. Later Palæozoic and Early Mesozoic faunas are either unknown or too poorly known in critical areas for it to be possible to form an estimate of their relationships. However, the late Palæozoic glaciation clearly indicates that there must have been climatic zoning at that time, while the recorded distribution of the fusulinid Foraminifera during the same interval would seem to indicate that the zones were more or less parallel to those now existing.

Summarizing, Prof. Durham stated that Tertiary marine faunas clearly militate against any position of the continents and poles markedly different from that now existing, and the available evidence for the Palæozoic is much more in accord with the present positions than those postulated for continental drift. In the subsequent discussion, Mr. R. A. Couper remarked that it is now known that the spores of the Permian plant *Glossopteris* are wind-distributed. Therefore, the geographical distribution of this plant over several continents could be explained without these having to be contiguous or joined by 'land-bridges'.

Dr. W. J. Arkell (Cambridge) discussed the possibilities and limitations of geology as a control in investigations of the type under consideration. Stratigraphical geology is always striving to break down the 'cinema film' of past periods into its component 'shots', and by means of correlation to obtain separate 'stills' for the whole world at as short intervals as possible. It has to be remembered that this process is still in its early stages. For some years Dr. Arkell has been collecting and sifting all the available information for one single system—the Jurassic—to see how many 'stills' could be obtained, and what sort of picture emerged for the world as a whole for this short stretch of 'film'. It represents approximately twenty-five million years. For correlation purposes, the approach had to be through marine faunas, and primarily based on the group of fossils that gives the best results, namely, the ammonites. Into the stratigraphical framework provided by the ammonites, such as it was, had then to be fitted any other evidence available—from corals, land faunas and floras, lithology, and so on.

After collecting and collating all such evidence, it began to be possible to discern faunal realms and provinces, and then to make deductions about climate in different parts of the Earth. One major mistake often made was to suppose that one such pattern of realms and provinces persisted throughout the whole Jurassic period. Actually, the frontiers were continually changing, and the patterns at the beginning and end of the Jurassic were completely different. How much such changes were due to climatic changes and how much to alterations in the distribution of land and sea it is very difficult to say: in any event, the two factors may have interacted considerably, major geographical changes producing climatic changes over large areas and even, possibly, the whole Earth.

One fact which stands out as of special importance in connexion with the present discussion is that at all times in the Jurassic (and Cretaceous) there was a rich and varied ammonite fauna in the Arctic Ocean. From its occurrence on all the lands at present bordering that ocean—in northern Siberia, Franz Josef Land, Spitsbergen, north-eastern Greenland and the Canadian Arctic—and its identity at any given moment in all those places, it is obvious that this fauna lived and bred in the Arctic Ocean, which therefore cannot have had an ice cap as at present.

In the Lower Jurassic the Arctic Ocean molluscan fauna is no different from that found in all the other

oceans of the world. There is one universal faunal realm. That the temperature was much more even than at present is proved also by the existence of rich flora in north-eastern Greenland and Grahamland, and by the fact that these floras scarcely differs from those found in Yorkshire and Sardinia.

Dr. Arkell is of the opinion that during the Middle Jurassic the uplift of land all around the Arctic Circle cut off the Arctic Ocean as a completely closed Mediterranean-type sea. At least, when the next transgressions began at the end of the Middle Jurassic and in the Upper Jurassic, a complete differentiated boreal fauna appeared for the first time and spread southward from the Arctic over the surrounding continental margins. Distribution maps show clearly that this fauna originated in the Arctic Ocean and continued to be centred there for the rest of the Jurassic, becoming progressively more differentiated, but sometimes retreating before, or mingling with, temporary advances of the equatorial faunas.

So it is obvious that throughout the Jurassic the Arctic Ocean, which is at present almost fully covered by permanent ice, was the breeding ground of a rich marine molluscan fauna secreting large calcareous shells, and in no wise more stunted or impoverished than their relatives in lower latitudes. In any event, no glacial deposits of any kind are known in the Jurassic. There are two possible explanations of this. Either the general temperature of the Earth was higher even than at present and, in particular, the polar regions were warmer, or the poles were in some other position. There is only one position where the poles could possibly have been in the Jurassic if they have to be assumed to be as cold as now: namely, the North Pole in the North Pacific and the South Pole in the South Atlantic. But even then, to get the North Pole sufficiently far south of rich Jurassic faunas in Japan and southern Alaska brings the equator too far north on the European side. The most likely position for the Jurassic equator is through southern Europe, following a belt of luxuriant coral reefs and sponge reefs. However, it is not possible to deny that the North Pole could have lain in the North Pacific during the Jurassic, because this is a region about which our knowledge is a complete blank, as is the corresponding position for the South Pole, in the middle of the South Atlantic. Moreover, it seems that if the poles were in those positions, it is quite likely that ocean currents would prevent ice caps forming, and that this would have a profound effect on world climates.

There is some evidence that the Arctic has been cooler than other oceans, as is shown by the scarcity of limestones and the absence of corals. If these facts are rightly interpreted, they suggest that the North Pole has been about where it is at present, but that the climate of the whole Earth was warmer and more even. We are then led to seek for a cosmic explanation, such as fluctuations in the amount of radiation received from the Sun.

Mr. T. Gold (Royal Greenwich Observatory) discussed the dynamics of the problem of polar wandering[6]. A movement of the entire Earth relative to the poles of rotation can be achieved by internal effects; only the vector of angular momentum has to remain fixed in space. If we have a stationary sphere resembling the Earth, a beetle weighing one gram could turn it over once by walking 10^{27} times around it. If instead we have a rotating sphere, then the beetle can cause any desired angle of movement of the poles relative to the sphere by merely sitting in the right place.

The actual Earth possesses greater stability than this by virtue of its shape. It is spinning around a principal axis of inertia of greater moment than the other two, and any departure from this condition implies an increase of energy, if the angular momentum is to remain the same. If the Earth were a perfectly rigid body, then no processes that we could suppose to have occurred could have caused a significant amount of polar wander. But, as it is only the stiffness of the spheroidal shape that gives it such stability, then, should there be no such stiffness over long periods, there would also be no stability.

Earthquake waves and tidal deformations, being effects that are very fast compared with the slow creep that is required to allow polar wander, cannot, therefore, be invoked. However, there exists a motion with a period of a little more than a year which has just the required effect of distorting the Earth as a whole, and thereby moving the axis of symmetry of the spheroid. This is the so-called 'free' or 'Eulerian' nutation, whereby the axis of rotation moves by a small angle, of the order of $\frac{1}{4}''$ relative to the Earth, with a period of about 14 months. Any plastic flow of the body of the Earth in such periods, in response to the forces resulting from this slightly misaligned spin, would show itself in a damping of the motion. In fact, the motion is heavily damped, such that it would decay, in the absence of disturbances, to $1/e$ in ten periods or less. But this damping can also be attributed to other causes. The fluid core of the Earth has been thought to be a source of the damping, but Prof. H. Bondi and Mr. Gold have shown that it is too small[7]. In the mantle, where the dissipative action must therefore take place, one can attribute it to certain types of non-elastic behaviour which do not result in permanent deformations. On the other hand, as the shape of the Earth is just the one appropriate to its present speed of rotation, this requires an explanation in terms of plastic flow. Also, plastic flow is the most common non-elastic behaviour of solids when they are heated to a temperature not far from their melting point. If, then, one attributes the damping to plastic flow, one knows the approximate magnitude of the effect. Here lies the problem: What effect would this amount of plasticity have so far as polar wander is concerned?

If we knew the nature and magnitude of a disturbance, such as a redistribution of mass on the surface, then we could calculate the rate and extent of polar wander which it would cause. A redistribution of mass would have the immediate effect of forcing the Earth to spin around a slightly displaced axis. The plasticity suggested by the free nutation is such as to cause the figure to be distorted in a direction whereby the shape will approximate to one appropriate to the new axis of rotation, at a rate of about 10 per cent per year. This is then responsible for a secular effect, for the axis of figure cannot be coincident with the axis of rotation unless it happens to be also a principal axis of inertia; a movement through a large angle might be required to achieve such a coincidence. The rate of the movement in relation to tectonic events can be illustrated as follows: if a continent the size of South America were raised by three metres, then it would take about a million years for the axes to swing around by an angle of the order of 90° to the new position of secular stability.

A movement of the poles on the Earth causes a certain amount of redistribution of mass in turn. The largest effect is the change in polar glaciation, and so in sea-level, and this can work in a sense either to help or to hinder polar wander. The most interesting effect would be connected with a hindrance, namely, the occurrence of a 'pole trap' where the induced changes of mass would counteract other disturbances and hold the axis until either a sufficiently large change occurred elsewhere, or until the distribution of land and water in the polar zone giving rise to the pole trap was substantially altered. If any such mechanism has been at work, then we must expect the polar wander during geological time to have occurred in a series of jerks rather than as a steady movement.

Mr. J. Parry asked whether the present small drift of the Pole could be attributed to any known dynamical processes, and Mr. Gold replied that the rate of melting of the Greenland ice gives an effect which is of the correct order of magnitude within the uncertainties of the data available. In answer to a question by Mr. B. C. Browne, Mr. Gold stated that continental drift would certainly imply the occurrence of polar wander, since, for the continents to slither around, so great a degree of plasticity is required that polar wander can take place very easily; polar wander, however, does not conversely imply continental drifting, because movement of the poles could occur in a much more rigid earth.

Dr. S. K. Runcorn (Cambridge) described the inferences which have been drawn from the direction of magnetization of those rocks the magnetic stability of which can be established with some degree of confidence. The fundamental hypothesis is that the Earth's magnetic field, averaged over a period of several thousand years, corresponds to that of an axial geocentric dipole. Thus it is necessary to sample from a sufficiently long stratigraphical sequence in order to eliminate the effects of secular variation and random errors. The hypothesis seems reasonable when the control exerted by the Coriolis force on the motions in the Earth's core is taken into consideration[8]. The palæomagnetism of Tertiary lava flows verifies the hypothesis for the Earth's more recent history[9], and the situation is unlikely to have been substantially different at any earlier time.

The palæomagnetic evidence that the polarity of the geomagnetic field has often been reversed many times within a geological period, without an alteration in the direction of the field, is further evidence that the axis of the field is fixed in the Earth in a fundamental way. Its coincidence with the axis of rotation therefore seems natural[9]. The palæomagnetic record prior to Tertiary times is fragmentary; but it is clear that, apart from reversals of polarity of the field, its mean direction has changed slowly through geological time. The simplest interpretation of this is to assume a wandering of the pole of rotation of the Earth carrying the mean magnetic pole with it. From the results of a palæomagnetic survey of sedimentary strata and lava flows undertaken by the Department of Geodesy and Geophysics at Cambridge, the motion of the Pole throughout the past six hundred million years has been traced in outline[10]. The examination of the palæomagnetism of rocks in other continents has not proceeded so far as in Britain, but the available data, shown in Table 1, are in approximate agreement with the British results.

Dr. Runcorn next considered an explanation of the reasonably steady movement of the pole of rotation from the mid-Pacific to its present position. The mechanism is required to have a correspondingly long time-scale. A lack of symmetry in convection currents in the Earth's mantle seems a possible cause; effects within the fluid core are unlikely to persist in the one direction long enough. It is probable that the equatorial bulge of the Earth adjusts itself to any new axis of rotation in a time very short compared with geological time. Thus, in discussing the dynamics of the problem, it is necessary simply to take axes fixed in the spheroidal earth and then to consider how much angular momentum needs to be generated in a certain part of the mantle to cause the remainder of the Earth to rotate through about 90° since later Pre-Cambrian times. A meridional convection current below the non-Pacific hemisphere rising to the south and falling to the north may be postulated, or one of opposite sense beneath the Pacific hemisphere, or several cells of differing intensities. The number of rotations made by such currents since Pre-Cambrian times depends largely on the amount of matter in motion, but has to be more than about fifteen. This corresponds to convective velocities of greater than 2 cm./year. In an earth of exactly spherically symmetrical properties, such a convection system could not occur. The persistent absence or comparative weakness of convection systems in part of the mantle since the Pre-Cambrian may, however, have also caused the present contrast between the largely continental nature of one hemisphere and the oceanic nature of the other, by affecting the extent to which the acidic rocks differentiated.

Mr. Gold commented that the effects he had described earlier depend on the basic fact that the Earth is rotating. The instantaneous effect of such a process as convection in the mantle is very much smaller than that of the process he had described.

Table 1. APPROXIMATE POSITIONS OF THE MAGNETIC NORTH POLE DURING GEOLOGICAL TIME
(Summarized from various sources, refs. 8–14)

Geological period	Great Britain	North America	Other Continents
Triassic	47° N., 133° E. (Keuper sands, ref. 11)	—	—
Permian	49°N., 162° E.* (Exeter traps, ref. 11)	—	30° N., 50° E.** (Basalt lava, New South Wales, ref. 10)
Pennsylvanian	—	43° S., 70° W. (Naco Sandstone, Arizona†)	—
Devonian	34° N., 156° E. (Old Red Sandstone, Wales, ref. 11)	—	
Silurian	—	20° N., 137° E. (Maryland, ref. 12)	
Cambrian	15° N., 173° E. (Caerbwdy Sands, Wales, ref. 11)	22° S., 153° W. (Tapeats Sandstone, Arizona†)	0° N., 130° W.* (Pilansberg dykes, South Africa‡)
		20° N., 165° E. (Keweenawan, Michigan, ref. 14)	
Late Pre-Cambrian	5° S., 131° W. (Upper Torridonian, north-western Scotland, ref. 11)	30° N., 148° W. (Hakatai Shales, Arizona†)	
Earlier Pre-Cambrian	37° N., 109° W. (Diabaig Group, Torridonian, north-western Scotland, ref. 11)	45° N., 99° W. (Diabase dykes, Michigan, ref. 13)	

* Mean based on such a small number of extrusive rocks that it is doubtful whether the effect due to secular variation is eliminated.
** Mean based on a single flow.
† Runcorn, S. K. (unpublished).
‡ Gough, D. I. (unpublished).

If the Earth were taken to be stationary, the convection currents would cause a slow rotation in the same manner as the beetle crawling around the surface. Prof. O. T. Jones welcomed the results of the palæomagnetic surveys. He stated that, from his recent studies of the red sandstones in the Devonian and Triassic, he had concluded that they have been derived from the erosion of tropical soils. This suggests that the equator, during the two periods, has lain near the British Isles—a conclusion which is in agreement with the implications of Dr. Runcorn's palæomagnetic data.

A. A. Day
S. K. Runcorn

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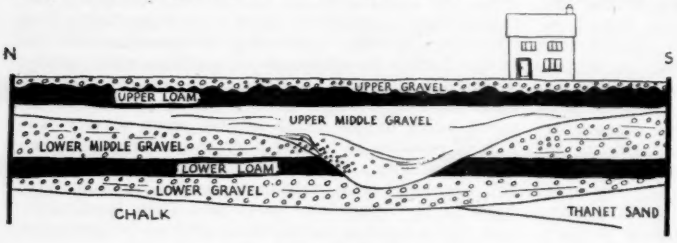
A FURTHER FRAGMENT OF THE SWANSCOMBE SKULL

By J. WYMER

THE right parietal of a human skull, found in the Middle Gravels at Swanscombe, Kent, on July 30 of this year, was discovered during excavations under licence from the Nature Conservancy (see *Nature*, Aug. 13, p. 287; for "Mr. Adrian Jones" read "Mr. Adrian Gibson"): a section was to be cleared from near the top of the Upper Middle Gravel, through the Lower Middle Gravel to the Lower Loam, for the purpose of studying the deposits and their archæological content. The spot chosen was close to the site of Mr. A. T. Marston's discoveries of 1935 and 1936, when he found the occipital and left parietal of Swanscombe Man. It is one of the few places in the reserve where undisturbed Middle Gravel deposits exist, although they are almost completely buried beneath dump and scree. Previous work in the pit for five years had proved the existence of this remaining piece.

The geology of the Swanscombe deposits is well known, having been carefully studied during the years of commercial excavation, when large exposures rendered the task more practical. Now there are only a few small patches where any of the undisturbed deposits can be seen. All these terrace deposits, except perhaps the topmost solifluxion layer, are of great interglacial age, possibly 250,000 years old, and they fill a large channel cut in the solid Thanet Sand and Chalk, almost a mile wide. When the River Thames flowed at this level, it meandered across its flood plain, scouring out new channels and filling in old. Such a channel remains 'fossilized' in the Middle Gravels of Swanscombe, the Upper Middle Gravel representing a channel cut into the Lower Middle Gravel and afterwards filled with gravels and sand. It was near the base of this channel, not far from its banks, that the Swanscombe Man fragments were found, in association with mammalian remains and flint artifacts datable to the middle period of the Acheulian flint industry. The complete section is illustrated diagrammatically in Fig. 1.



AFTER HINES, KING AND OAKLEY

Fig. 1

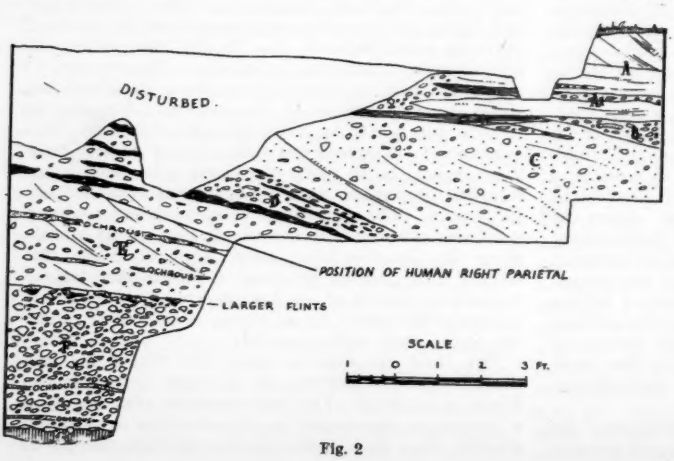


Fig. 2

The channel of Upper Middle Gravel is obviously more recent than the Lower Middle Gravel below; but they both contain a flint industry of Middle Acheulian type. Some authorities consider the Upper Middle Gravel flint industry to be more evolved; but this is debatable. The surface level is 110 ft., and the bottom of the main channel attains a depth of 40 ft.

A thick capping of overburden and the lack of an existing face made it necessary for the excavation

to be conducted downwards by horizontal planes in order to determine the lay of the strata. The current-bedded sand at the top of the excavation was recognized as part of the Upper Middle. It was devoid of bone or flint artifacts; but a thin seam of gravel running through it (*A*1 in Fig. 2) yielded numerous flint flakes, as did the thicker seam of gravel below (*B*), which also contained a small hand-axe, two miniature cores and some bone fragments, as yet unidentified. The very loose sand with stones beneath contained few artifacts and no bone. Layers *A*1 and *B* were approximately horizontal in a west-north-west–east-south-east direction, but at right angles showed a slight dip to the south-south-west.

On the second day of the excavation, in the west-north-west end of the section, a thin band of very ochrous gravel was revealed with unstained gravel beneath it. This had a steep dip to the east-south-east and south-south-west. Immediately, several bone fragments and flint flakes were found. Further removal of material showed more inconsistent bands, also dipping but tending to converge upon the east-south-east. The gravel continued to be very rich in flint artifacts and bone fragments; many of the flint flakes were lustreless and sharp, as if freshly struck, others were slightly rolled. Excavation in this region was very difficult because of the proximity of thick, loose overlay, which tended to fall. A thicker band of dark, stained gravel was exposed beneath, and as this was being examined a small piece of bone showed through the lighter gravel at its base. The surrounding material was cleared away by the fingers and the new find, a right parietal, was exposed *in situ*, dome upwards. Its upper surface, at least, was seen to be in soft, soapy condition, and ancient cracks already existed. It was decided to cease all further work in order to decrease the probability of a fall of the disturbed material upon it, but to leave the bone *in situ*. Photographs were taken and a temporary protection of boards placed over the parietal in case of a dangerous fall; Mr. Marston arrived two hours later and confirmed the discovery. A protective covering of plaster was made over the bone; but the loose nature of the gravel and the existing cracks defied attempts to extricate the fragment complete. All the pieces, however, were retrieved without damage to them and packed carefully in a box that was not opened again until arrival at the British Museum (Natural History) on August 2, save for the placing of a saturated wad of cotton-wool above to prevent any serious drying out.

Fig. 2 shows the conclusion of the excavation with the Lower Gravel (*F*) exposed, resting upon the tenacious Lower Loam (*G*).

Mr. Marston confirmed that his occipital bone was found 51 ft. to the west-north-west of the new find and 49 ft. from the left parietal, the position of the three fragments forming a triangle with sides 51, 49 and 24 ft. In his report on the discoveries (*J. Roy. Anthrop. Soc.*, **67**, 350; 1937), his geological section agrees exactly with the new one figured, showing four oblique ochrous layers and, of great significance, the bottom one as the bone layer. The new fragment was barely one inch below this seam in the unstained sandy gravel, a difference of no importance in a river deposit. The only item of interpretation in which the new section differs from Mr. Marston's is that the layer of larger flints at the top of the gravel marked *F* is considered to be the junction of the Lower and Upper Middle Gravels. Mr. Marston features the large flints but accepts the bottom ochrous band (the bone layer) as the junction. The gravel of *F* is more compact and darker than that of the looser, lighter gravel above it (*E*), which contained bone fragments. No bone fragments were found in *F*.

A comparison of the two sections shows that there can be no doubt that the new right parietal was found in the same stratum as that in which Mr. Marston found an occipital and left parietal in 1935–36. This renders the fact that all three fragments fit each other perfectly, belonging to the skull of the same individual, a little less remarkable.

Numerous bone fragments were found in association with the right parietal, and these are now at the British Museum for purposes of hardening and identification. It is hoped that they may add to the list of fauna from this level. Almost three hundred primary and finishing flint flakes were found during the excavation, of which only twelve showed signs of possible utilization. Four small, primitive hand-axes were also found, typical of the Acheulian flint industry as represented at Swanscombe, as well as the point of a larger, finely finished hand-axe and a magnificent flake 'knife'. A full account of these artifacts will appear in the forthcoming report. Most of them were in a very sharp condition, with only a few minute chippings along the edge and a lustre from rolling in the water. Many, however, are lustreless and virtually in mint condition. Although separated, the skull fragments show very little abrasion from rolling and there can be no doubt that the flint industry is contemporary.

Inside the triangle formed by the position of the three skull fragments recovered remains a mound, part of which is definitely undisturbed gravel of the skull zone. This area is now efficiently protected, and is to be excavated in the near future.

USE OF RADIOACTIVE MATERIALS IN BIOLOGICAL ASSAY

A JOINT meeting of the Society for Analytical Chemistry, the Royal Institute of Chemistry, the Society of Chemical Industry and the Chemical Society was held during July 11 and 12 in the Biochemistry Department of the University of Edinburgh; the purpose of the meeting was a symposium on "The Use of Radioactive Materials in Biological Assay". Nine papers were read by invited speakers, and the programme also included a dinner and a visit to the Institute of Seaweed Research.

Public attention now being focused on the peaceful uses of atomic energy and its by-products, it was perhaps not inappropriate that members of four learned societies should meet for the purpose of discussing some of the latest applications of the isotope technique to biological problems. Some speakers, however, had obviously found difficulty in fitting the subject of their paper to the rather narrow field where biological assay and the applications of radioactive isotopes overlap. That the meeting was a success was in the main due to their interpreting the title of the symposium somewhat widely.

After a brief introductory address by Prof. G. F. Marrian, in which he recalled the pioneer work done

in the field of radioactive tracers by G. von Hevesy and his collaborators, the present writer gave an introductory paper on the determination of radioactive isotopes in biological samples. For the benefit of those not familiar with the instruments available for this purpose the first part of the paper was elementary in character, and was followed by an account of the more refined technique of gas-counting, by means of which the isotopes of carbon and hydrogen can be determined with high precision when present together in the same sample. A lively discussion followed, and most members present showed that they had an intimate knowledge not only of the virtues but more particularly the vices of many of the types of counter available. During the discussion on gas counting, Mr. F. P. W. Wintringham pointed out that ionization chambers used in conjunction with the most recent types of d.c. amplifiers are ultimately more sensitive than counters, but that they probably require more skill to operate and are not suited for use by assistant staff.

Dr. E. Lester Smith, of Glaxo Laboratories, Ltd., followed with a paper on the principles of isotope dilution assays. He exemplified the use of this valuable technique by means of an imaginary application to the determination of sodium chloride in sea water, and then enumerated the essential criteria for successful application of the technique. The most important of these is the isolation of a pure sample of the compound to be determined, once mixing with the labelled material is complete. This is sometimes very difficult, recrystallization to constant specific activity being often a laborious procedure. A useful application to the vitamin B_{12} field is the determination of the vitamin in crude fermentation liquors. These contain too many interfering substances for colorimetric or microbiological methods to be easily applied, but the problem has been solved by the use of vitamin B_{12} labelled with cobalt-60. A known amount of known specific activity is added to the fermentation liquors, and a pure sample of the B_{12} is isolated using resin-column chromatography. Ultimately the purified vitamin B_{12} is counted in a liquid counter. The chief disadvantage of the method is the four-step purification procedure. In the course of his paper, Dr. Lester Smith also referred to more sophisticated applications of the technique, mentioning in this connexion the use of labelled reagents, for example, iodopiperonyl chloride labelled with iodine-131 for the determination of amino-acids in protein hydrolysates. He also referred to the reversed isotope dilution technique, in which the total amount of a labelled constituent of a mixture is determined by adding a known weight of unlabelled substance, re-isolating it and determining its specific activity. From this and from the weight added, the total activity of the substance in the mixture can be calculated. This is sometimes referred to as the carrier technique, and as such was more familiar to some members of the audience.

Another practical application of the technique to what might be termed a commercial problem was described by Mr. G. C. Ashton, also of Glaxo Laboratories. This problem is the determination of penicillin G (benzylpenicillin) in fermentation liquors. These contain also other penicillins and other antibiotics which virtually rule out microbiological assay. Methods are available in the literature for the preparation of deuterium- or ^{13}C-labelled penicillin G suitable for isotope dilution applications; but the deuterium-labelled penicillin in particular is too costly and laborious to produce with a sufficiently high isotopic abundance for practical applications. (This consideration does not, of course, apply to tritium, which is not only cheaper than deuterium but also cheaper than carbon-14.) In the method described, the ^{14}C-labelled material is prepared by standard procedures. Known activities are added to the fermentation liquors and penicillin G isolated and its specific activity determined. It was in the accuracy claimed for his radioassays that Mr. Ashton provoked most discussion, his claims for closer than 5 per cent agreements between replicates being questioned by some speakers. His assertion that the counting rate of thick samples continues to increase beyond the usually accepted infinite thickness value was confirmed by Dr. W. F. Pover, of Birmingham, and was attributed to edge effects.

Dr. A. S. McFarlane, of the National Institute for Medical Research, Mill Hill, gave an account of the preparation and research applications of radioiodinated plasma proteins. It has been found that if the protein molecules contain not more than ten atoms of iodine per molecule, they are unchanged antigenically. More than ten atoms iodine per molecule produces an antigenically different protein with different crystalline properties. When radioiodine is used, it is not sufficient to have an *average* of less than ten atoms iodine per protein molecule: steps must be taken to see that no appreciable proportion of the molecules contains more iodine than this because, having different chemical properties, and, in consequence of the high iodine content a relatively high specific activity, their presence will give rise to erroneous results. The practice at Mill Hill, where radioiodinated proteins are prepared, is therefore to use iodine-131 in the proportion of one atom per protein molecule, and to carry out the reaction under conditions that will ensure uniform reaction. Proteins react very rapidly with iodine at alkaline *p*H, one molecule of hydrogen iodide being eliminated for each iodine atom attached. Fast mixing is therefore necessary, and this is achieved by means of a microjet apparatus in which jets of protein solution and iodine solution flowing in opposite directions meet in a minute mixing chamber. The yield is usually of the order of 60 per cent of the theoretical, and the product is purified on an ion-exchange resin. The final solution as made available for research is isotonic, neutral, sterile and with less than 1 per cent of the radioactivity in the form of iodide ion.

Experiments have been carried out to ensure that the labelled plasma proteins so prepared cannot be 'recognized' by experimental animals. This work has been done by comparing the specific activity – time curves of radioiodinated proteins and biosynthetic ^{14}C-labelled proteins. These were prepared by feeding high-specific activity ^{14}C-algal protein to rabbits and then isolating the plasma proteins. It was found that the two curves nearly coincide, the difference between them being of very doubtful significance. A useful property of the radioiodine-labelled proteins is that the label is not re-utilized as is carbon-14. Whereas the metabolic products of ^{14}C-labelled proteins are re-incorporated into new proteins, the iodine-131 is rapidly excreted once the protein has been degraded. This has the practical advantage that only the protein administered is being traced.

The application of an isotope technique to the assay of aldosterone and other steroids was described in a paper by Mr. R. N. Jones, Mrs. S. A. Simpson

and Dr. J. F. Tait, of the Middlesex Hospital, London, and read by Dr. Tait. The assay method is based upon changes in the sodium–potassium ratio in adrenalectomized rats caused by certain adrenal hormones, notably deoxycorticosterone and aldosterone. Two short-lived isotopes, sodium-24 and potassium-42, of half-lives 15·06 and 12·4 hr., respectively, can be employed for this purpose. The technical problem which has to be met in this work is either the measurement of each isotope separately in the presence of the other, or the measurement of the ratio itself. This latter alternative has been found by these workers to be the better, and the method used is based on the fact that, whereas an absorber of thickness 300 mgm./cm. placed between the sample and the counter reduces the observed counting rate due to sodium-24 by a factor of 20, it reduces that due to potassium-42 by a factor of only 3. This fact can be applied successfully to the measurement of sodium-24/potassium-42 ratios, and carries with it the advantage that whereas both isotopes decay rapidly, the ratio does not because they both have very nearly the same half-life.

The assay is carried out by reference to a graph of the logarithm of the ratio plotted against the logarithm of the dose of steroid. Aldosterone is found to be 120 times more potent than deoxycorticosterone, which is used as a standard. The method, although very successful, was compared by Dr. Tait with a non-isotopic method, namely, the measurement of sodium–potassium ratios by flame photometry, and the opinion was expressed that in the long run the non-isotopic method would probably prove to be preferable.

Isotope dilution methods for the determination of cortisol and aldosterone, using the technique according to the principles described by Dr. Lester Smith, were also described. Ring-labelled cortisol and aldosterone di-(1-^{14}C)-acetate were used. The chief difficulty was in the purification of the extracted compounds, column chromatography being used.

The use of iodine-131 for another bio-assay was described by Mr. Trevor Kinnear of Edinburgh, whose paper was concerned with the assay of thyroid-stimulating hormone. His method is based on the effect of the hormone on clearance of labelled iodine from the thyroids of day-old cockerels. The chicks are injected first with iodine-131 and then, for several days following, with the sodium salt of thyroxine. Control groups are then injected with saline and other groups with various amounts of the hormone preparation to be assayed. The change in activity of the thyroids as measured *in vivo* indicates the rate of clearance of labelled iodine and is a measure of the activity of the thyroid-stimulating preparations.

Another method in which radioiodine is used was described in a paper by Mr. F. W. Jennings and Dr. W. Mulligan, of Glasgow. Their problem is to measure the volume of intracellular plasma in centrifuged red cells. Hæmatrocrit values as determined by the conventional methods are found to give very variable results, especially with the blood of cows and sheep. The isotope method adopted was based on the use of plasma proteins labelled with iodine-131 and subsequent measurement of the residual activity in the centrifuged cells. Variable results were still obtained with cows and sheep; but these were traced to the effect of temperature and time of centrifuging. More uniform results were obtained by the use of cooled centrifuges, the temperature being controlled at 15°.

Mr. F. P. W. Wintringham, of the Pest Infestation Laboratory, gave an account of some of the methods employed for investigating the biochemistry of toxic action in insects. His experimental animal is the common house fly, and at least one of his audience envied him the convenience of the size of his subjects, and admired the technique devised for experimenting upon them. Mr. Wintringham first pointed out that, in general, there are many dangers attendant upon the use of *in vitro* techniques, especially when toxic action is to be investigated. A given agent may inhibit particular enzymes *in vitro*, whereas in the living animal it may either never reach that enzyme or it may reach it by pathways which are absent in *in vitro* preparations. Since the well-being of the whole animal depends on the existence of metabolic pools the components of which are each within a particular range of concentrations, toxins cause accumulations or depletions of various components of these pools. It is to the study of metabolic pools and the action upon them of certain known toxins that an isotope technique coupled with paper chromatography has been successfully applied.

Mr. Wintringham took for his example the labelling of metabolic pools with phosphorus-32. When carrier-free ^{32}P-phosphate is administered to the fly, all the metabolically active phosphorus-containing organic compounds become rapidly labelled. The fly is then killed and tissue extracts separated on paper chromatograms, and their presence and specific activities determined by scanning techniques. The effect of toxins on the labelling pattern is often spectacular. For example, the effect of methyl bromide is a large diminution in the size of the adenosine triphosphate pool and an increase in the size of the inorganic phosphorus pool. *Cyclo*propane, on the other hand (which is used as an anæsthetic during the injection of the flies), has no such effect. An interesting refinement of the technique is the confirmation of the specific activity of the phosphorus compounds: the original phosphorus-32 activity is allowed to decay away and the papers are irradiated in the pile, when all the phosphorus in the spots becomes radioactive by the $^{31}P(n,\gamma)^{32}P$ reaction, thus giving, by its radioactivity, a measure of the amounts of phosphorus in the spots as would have been found by a chemical determination. By this means the interesting fact was confirmed that all the phosphorus atoms of adenosine triphosphate are labelled when phosphorus-32 is injected into house flies, whereas in mammalian tissues only the β- and γ-atoms are usually found to be radioactive. In answer to a question, however, Mr. Wintringham expressed the view that this does not constitute an important qualitative difference between the metabolism of insects and mammals, but is merely a reflexion of the higher turnover-rates in insects. He believes that, given time, all the phosphorus atoms of adenosine triphosphate in animal tissues would be exchanged; in fact, the similarities between the biochemical pathways of rats and flies are more striking than the differences.

The concluding paper of the meeting was a survey by Dr. J. F. Loutit of the dangers to health entailed in the manipulation of radioactive substances. In general, he was reassuring: only activities greater than 1 mc. of γ-ray emitters need be regarded as a danger so far as external radiation of workers is concerned, and these dangers can be controlled by the use of pocket dosimeters or film badges. Blood counts are much less satisfactory than such physical

methods of measurement. Blood counts remain more or less constant until the subject has had a sufficient dose to produce irreversible damage and no institution in Great Britain now relies solely on this method of control. The dangers from internal radiation, which arise only when radioactive materials are inhaled or eaten, in general entail similar risks: that is to say, quantities of the order of 1 mc. inside the body are to be regarded as dangerous. But here, of course, the chemical properties of the isotope, which determine where it is deposited and how fast it is eliminated, its half-life and the nature of its radiations are also important considerations in assessing the dangers. The bone-seeking isotopes such as strontium are by far the most dangerous, as they irradiate the bone marrow. In concluding, and in answer to questions, Dr. Loutit expressed his opinion that emitters of soft β-radiation such as carbon-14, tritium and sulphur-35 are not particularly dangerous, and precautions similar to those taken by bacteriologists when dealing with pathogenic organisms would protect workers. He reminded the audience, however, that whereas pathogenic organisms accidentally let loose in the laboratory can be destroyed by disinfectants or other agents, radioactivity cannot be destroyed.

In attempting to assess the value of the meeting, the present writer is under a difficulty. A symposium devoted to technique rather than to a field of research brings together workers on very different subjects, whose common interest hangs on the somewhat slender thread of common technique. A smaller proportion of those attending the meeting are therefore able to comment pertinently on each piece of research than occurs when the subject is common to all. There can be no doubt, however, that the meetings were much enjoyed by those present.

R. F. Glascock

NEWS and VIEWS

Textile Chemistry at Manchester: Prof. R. H. Peters

The Manchester College of Technology has trained students of textile chemistry ever since 1905, and the pioneer work of Prof. Knecht, of J. Huebner and of F. Scholefield is well known. The industry has always found great difficulty in recruiting skilled technologists, and it has sometimes failed for years to make full use of discoveries which have been made by the big firms and by research associations such as the Shirley Institute and the British Rayon Research Association. It is possible that if the textile industry of Britain could exploit the new synthetic fibres and the new methods of dyeing and finishing, it might have a chance of re-establishing something of its former position of pre-eminence in those world markets in which it cannot compete so long as it restricts itself to traditional fibres and the old-fashioned techniques which are now so well known to its competitors.

The appointment of Dr. Raymond Harry Peters to be the first professor of textile chemistry in the Manchester College of Technology may therefore prove to be of great importance to the textile industry as a whole. Dr. Peters obtained the degree of bachelor of science with first-class honours in chemistry at King's College, London, in 1939 and took his Ph.D. in 1942. He was in the Dyestuffs Division of the Imperial Chemical Industries, Ltd., at Blackley during 1941–47. He took a degree in mathematics in the University of Manchester in 1949 and then returned to Blackley. He became head of the Dyeing Research Section of I.C.I. in 1952. He has published a number of scientific papers and in 1953 he was awarded the medal of the Worshipful Company of Dyers. Prof. Peters will have at his disposal a splendid new dyehouse which has been very well equipped, thanks to the generosity of Courtaulds and the co-operation of a number of firms, in particular Messrs. Mather and Platt, Ltd. He should be able to build up a strong postgraduate research school in co-operation with local industry, and it is much to be hoped that his department will attract students who can be trained in this vitally important branch of technology, and so help the textile industry of Britain to bring itself up to date.

Electrical Engineering at Swansea: Prof. W. Fishwick

The chair of electrical engineering which was recently established at the University College of Swansea has been filled by the appointment of Dr. W. Fishwick, who is at present a lecturer in engineering in the University of Edinburgh. After obtaining first-class honours in the Mechanical Sciences Tripos at Cambridge in 1940, together with the Wynne Prize from Pembroke College, Dr. Fishwick joined the staff of the Telecommunications Research Establishment, where he was concerned first with the development of radar equipment and later with radio counter-measures. In 1944 he was commissioned for special duties with the R.A.F. and commanded a scientific intelligence unit operating in North-West Europe. Dr. Fishwick returned to Cambridge in 1946 as a research student under Prof. E. B. Moullin and Mr. C. W. Oatley, where he developed a novel type of mass-spectrometer using radar techniques which was capable of following very rapid changes in gas composition. Upon the completion of this work, for which Dr. Fishwick was awarded the degree of Ph.D., he took up a post with Imperial Chemical Industries, Ltd., at Manchester in 1948, where he was placed in charge of a laboratory for the development of instruments concerned with the automatic control of chemical plant. Two years later he was appointed to his present post at Edinburgh. Dr. Fishwick's more recent researches have been concerned with automatic control theory and with certain machine-tool vibration problems. It is peculiarly appropriate that the first occupant of the new chair should be actively interested in the field of automatic control, for many of the steel, chemical and other plants which are situated in the immediate neighbourhood of Swansea make extensive use of such controls.

Social Responsibilities of Scientists

In a paper presented to the American Philosophical Society (*American Scientist*, **43**; July 1955), Prof. J. H. Hildebrand, of the University of California, examines the social responsibility of scientists. They should first try to extend the boundaries of knowledge and be able to answer Benjamin Franklin's question,

"Do you love truth for truth's sake, and will you endeavour impartially to find and receive it for yourself, and communicate it to others?" They should in no way be blamed for not foreseeing every evil use to which their discoveries might be put; "those who decry science should come out into the open and say they prefer ignorance to understanding, darkness to light". Outside their scientific discoveries the mass of scientists are like the mass of other people with all the same human weaknesses and strengths. They should bear their moral responsibilities for the use or abuse of scientific discoveries with the rest of mankind.

City and Guilds of London Institute

In the seventy-fifth annual report of the City and Guilds of London Institute, attention is directed to the launching of the ninth examining body, now officially known as the Associated Examining Board, for the General Certificate of Education. The establishment of the Board has been welcomed as one of the most significant developments in secondary education in recent years and a development which should undoubtedly prove to be of far-reaching importance in both educational and industrial fields. The new body is the only one of the nine now examining for the General Certificate of Education on which industrial and professional interests are directly represented, and the Board has therefore had in mind the production of syllabuses and the introduction of new subjects suitable for students intending to enter industry and commerce. The Council also reports progress in the number of candidates taking City and Guilds examinations, and of further requests for training courses. The number of candidates who sat for the Department's examinations in 1954 totalled 88,560—an increase of 6,273 over the previous year. Many new subjects were introduced during the year and major revisions of existing schemes were undertaken.

In the City and Guilds College the numbers of full-time undergraduate and postgraduate students have risen considerably; a chair of heavy electrical engineering and a readership in production engineering have been instituted, to be financed from the income which will be received from a share of the Counterpart Funds placed at the disposal of the University of London; and the first allocation of special expansion recurrent grant will allow for the appointment of a professor of applied science (engineering) and a professor of soil mechanics, in addition to other necessary additions to the lecturing staff.

British Hat and Allied Feltmakers Research Association

The seventh annual report of the Director of Research of the British Hat and Allied Feltmakers Research Association (pp. 19+2 plates; from the Association, Manchester, 1954) covers the year ended August 1954 and refers to the fresh conditions of grant obtained for the five years commencing in April 1954. For the first two years the government grant remains unchanged, but for the following three years the industry is asked to raise an annual minimum of £12,000 to attract grant at the rate of £9,000 a year, with up to a further £5,000 on the basis of £100 for each £100 in excess of the stipulated minimum raised by the industry. One of the Association's outstanding achievements has been the development of equations for carrotting fur, although further work and factory trials will be necessary to establish their validity for all grades of fur and different types of carrotting reagents. So far, different equations have been established for the standard 'mercury in nitric acid' reagent and for non-mercuric reagents of the 'hydrogen peroxide–nitric acid' type. The study of wool noil, of the directional felting of wool and the physical properties of fibres continued, and the operation of multi-roller machines in relation to the degree of carrotting and the grade of fur used was investigated. The survey of wool bumping machines has yielded information on the loading of crank bumpers in relation to the wetness and temperature of the mass, and to the rate of felting, which should find immediate application in the industry, while observations on the time of dyeing of wool felt were immediately reflected in the quality of the hats produced. *Ad hoc* research programmes included the blending of fur with protein fibres, the shower-proofing of hats and the stoving temperature in carrotting fur. Other work has been concerned with standardization of raw materials and finished products. A research conference held in conjunction with the annual general meeting, at which papers reviewing the work of the Association were delivered, was an outstanding success.

Finnish Institute for Occupational Health

The Board of Trustees of the Institute of Occupational Health in Helsinki is made up of members of the Cabinet, the universities and voluntary organizations. Its funds are drawn from industry, insurance companies and workers' organizations and, each year, it publishes a report in English (Occupational Medical Foundation and Institute of Occupational Health. Annual Report, 1954. Pp. 47. (Helsinki: Occupational Medical Foundation and Institute of Occupational Health, 1955)). Besides a general department, it has medical, physiological, psychological and industrial hygiene departments and, in 1954, the number of workers engaged on research projects increased substantially compared with the previous year. Among the original investigations carried out was one into the problem of sickness absenteeism among government workers. Of 44,000 government workers whose cases were examined, the frequency of sickness among groups working indoors has been three times higher than that among outdoor workers. A study of the characteristics of an influenza epidemic and of the effects of immunization showed that previous attacks of influenza had no clear immunizing effects, while the results obtained from immunization were unsatisfactory. Investigations were also made into the reasons why javelin throwers have to give up their sport each year for varying periods, while a statistical study was also made into the relation between age and number of traffic accidents incurred by some five hundred bus drivers. This showed that the number of accidents involving drivers with long experience diminishes with age up to approximately 45–48 years, after which the accident-rate begins to grow.

Macaulay Institute for Soil Research: Report for 1953–54

The annual report for 1953–54 of the Macaulay Institute for Soil Research, Aberdeen (pp. 55+2 plates; from the Institute at Craigiebuckler, Aberdeen; 1955), indicates that the Institute continues actively to promote its aim of advancing the fundamental study of the soil in all its aspects. The Pedology Department surveyed a further 260 square

miles of Scottish soils, and continued the study of conditions and practices on upland grazings with special regard to heather management. Improvements of existing analytical methods are reported by the Department of Spectrophotometry, and the use of a double-beam infra-red spectrometer has assisted the positive identification of some substances present in soil organic matter. Investigations on the carbohydrate and nitrogenous constituents of the soil have been continued by the Department of Soil Organic Matter. Other activities of this Department include studies in the transformation of fatty materials in the soil by Actinomycetes, the physiology of aerobic thermophils, lignin decomposition studies and work on peat classification, including pollen analysis. The Department of Plant Physiology, working with sugar-beet disks, finds that phosphate absorption is related to the reducing-sugar content of the cell. It is also suggested that the ratio of phosphorus to iron in leaves gives a clearer picture of the iron status of plants than the individual concentration of these elements, high ratios being characteristic of iron-chlorotic plants. Investigations of soil fertility directed towards improving fertility practice, clarifying the significance of soil properties in relation to crop production and developing methods for evaluating the nutrient status of soils, have been continued.

Journal of the Institution of Telecommunication Engineers, India

The first number of the *Journal of the Institution of Telecommunication Engineers, India* (see *Nature*, July 16, p. 100), was published in March, and for the present it will be produced as a quarterly (from the Institution, Curzon Road, New Delhi; Rs. 15 a year or Rs. 5 per copy). It is intended that each issue of this *Journal* will normally comprise an editorial note, two or three research papers, articles of general interest for students, reviews of papers, news and notes, and letters to the editor. The first number contains the texts of the addresses delivered at the inaugural session of the Institution on November 2, 1953: first, the address of welcome by A. C. Ramchandani, chief engineer of All-India Radio; next, the inaugural address by Dr. S. Radhakrishnan, vice-president of the Republic of India; and then the presidential address by B. R. Batra, chief engineer of the Posts and Telegraphs Department. Mr. Batra's second presidential address, delivered on December 12, 1954, is also reproduced. The more technical material published in this issue comprises an article on line fault location by pulse technique, by T. K. Mahadevan; an account of radio measurements at Jabalpur during the solar eclipse of June 30, 1954, by members of the Government Engineering College at Jabalpur; and a paper by S. Sampath on power density diagrams of short-wave aerial arrays. The *Journal* concludes with the annual report of the Council of the Institution for 1953–54, and a list of current members.

Anguinomorph Lizards

In 1878 Steindachner described a lizard from Sarawak which he named *Lothanotus borneensis*. Only a few specimens have been collected since then, so that it is still rare in museums. As it was regarded as closely related to *Heloderma*—indeed, it was sometimes put in the same family—it presented a remarkable example of discontinuous distribution. S. B. McDowell, jun., and C. M. Bogert in "The Systematic Position of *Lothanotus* and the Affinities of the Anguinomorph Lizards" (*Bull. Amer. Mus. Nat. Hist.*, 105, Art. 1, 1954; pp. 142+43 figs.+6 plates; 6 dollars) started to examine this problem but were soon led into wider fields and examined the crania of 306 living reptiles and those of eight related fossil genera. This work is illustrated, *inter alia*, with the detailed drawings of the skulls of twenty-eight species of reptiles, and as the majority of these are original they furnish a very valuable series for reference. These and the other text-figures are excellently drawn and well reproduced. In addition to the claim that *Lothanotus* is allied to *Heloderma*, Nopcsa suggested that it also resembled *Shinisaurus crocodilurus*, another rare lizard from southern China, and this point is also specially investigated. The procedure adopted is to give full accounts of the external features, skulls and post-cranial skeletons of these species and of the classical lizard genus *Varanus*, and then to make detailed comparisons between them and a series of key forms. Among the conclusions reached is that the Anguinomorpha, as here defined, include the extinct Mosasauridae and constitute a natural infra-order divisible into two superfamilies, the Diploglossa and the Platynota. *Shinisaurus* belongs to the former and, although *Heloderma* falls in the latter, it is in a separate family not closely related to the Lothanotidae. Some of the characters of *Lothanotus* are such as might be expected in a form ancestral to snakes. It is further suggested that the Typhlopodidae should not be included in the Ophidia but should be regarded as a highly specialized line of Anguinomorph lizards the resemblances of which to the snakes result from convergence. Altogether, this is a most useful contribution upon which the authors are to be congratulated.

Biological Races of *Phytophthora infestans* in Canada

In recent investigations of the *Phytophthora* blights of tomatoes and potatoes in eastern Canada, it was found that different isolates of the fungal pathogen did not always yield reproducible effects in inoculation experiments. This led to the view that different biological races of the fungus might be involved—a view fully borne out by further experimental work now reported by K. M. Graham and H. N. Racicot (Soc. de Quebec pour la Protection des Plantes, Thirty-fifth Rept., p. 50; 1953). Thirty-one isolates of *P. infestans* from potatoes were examined. Cytological study showed that the zoospores are uninucleate and that genetically pure lines could therefore be obtained, these being cultured on oatmeal agar. On a host-range comprising both potato and tomato varieties, it was found in comparable inoculation tests that three races of the pathogen could be differentiated, their respective effects being indicated.

Photosynthesis in Different Phyla

L. Norris, R. E. Norris and M. Calvin have reported on a survey of the rates and products of short-term photosynthesis in plants representative of nine different phyla (*J. Exp. Bot.*, 6, 16, 64; 1955). When short-term experiments on photosynthesis, using carbon-14 dioxide and paper chromatography, were performed with twenty-seven different plants representing nine phyla, there was a remarkable uniformity in the types of ethanol-soluble compounds which became radioactive in the entire group of plants used. The percentage amounts of the different compounds varied considerably among the various

(*Continued on p.* 447)

BRITISH ASSOCIATION MEETING AT BRISTOL

SCIENCE AND THE SCIENTIST*

By Sir ROBERT ROBINSON, O.M., F.R.S.
President of the British Association

THE Association held its sixth annual meeting in Bristol in August 1836 under the presidency of the Marquis of Lansdowne. No significance need be attached to the fact that the well-known riots occurred here in the following year, which is much better dated by the accession of Queen Victoria. I noted from the report with interest and a certain wistful approval that the address was delivered on that occasion not by the president but by Charles Daubeny, Fellow of Magdalen College, Oxford, local treasurer for Oxford. Daubeny had already been appointed to the University chairs of chemistry and of botany. He was well qualified to profess geology also; but his eventual third chair was that of rural economy. He was a founder member of the Association and had attended the five previous meetings. Undoubtedly one of the chief pioneers and benefactors of science in Oxford, he was a man of great originality and tireless energy. Among many notable contributions to knowledge, one that deserves the epithet classical was his discovery that the process of photosynthesis occurred more rapidly under the influence of blue and violet light than under that of the less refrangible rays.

In the course of his address, Daubeny referred, among other things, to various systems of notation in chemistry and mentioned in that connexion Dr. John Dalton, whom he described as the 'venerable' founder of the atomic theory but who was in fact just short of seventy years of age. At the meeting of the Association in the previous year at Dublin, Dalton had given an account of his experiments on the distillation of caoutchouc and of the formation of a volatile oil which was doubtless crude isoprene. This he called 'double olefiant'. One volume of the vaporized substance exploded with six volumes of oxygen gave four volumes of carbonic acid gas, which works out for:

$$C_5H_8 + 6O_2 = 4CO_2 + 3H_2O + (CH_2O).$$

Here the CH_2O merely represents loss of partly oxidized hydrocarbon. I mention this matter, rather out of context, because the work seems to have been forgotten.

Dalton was not too 'venerable' either to be a vice-president of the Chemistry Section at the Bristol meeting, or to be charged with the duty of examining the lesser constituents of the atmosphere. One wonders whether the small incombustible residue noted by Cavendish, and which was undoubtedly argon, was known to him.

The next meeting at Bristol was in 1875. The president was the eminent engineer Sir John Hawkshaw, and he delivered a most interesting address on the historical development of achievement made possible by applications of engineering and other sciences. The last three sentences will interest you. "... Need we try to extend our vision into futurity further? Our present knowledge, compared to what is unknown, even in physics, is infinitesimal. We may never discover a new force—yet, who can tell?"

The meeting was attended by many famous men of science, and among the officers of sectional committees we find Cayley, Thomson (afterward Lord Kelvin), Tyndall, Rayleigh, Williamson, Galton, Armstrong and Tilden; a short list at random that could be greatly extended.

Sir William Crookes was the president at the third Bristol meeting, in 1898, and he delivered an outstanding address on a variety of topics. First on the gloomy prospects for the supply of bread to the people of the British Isles. Here he foresaw with characteristic intuitive genius that the only possible solution would be to obtain nitrogenous products from the atmosphere. At that time the only known method was the combustion of nitrogen with the help of a powerful electric discharge, an experiment initiated by Henry Cavendish, developed by Crookes himself in 1892, and by Rayleigh for the isolation of argon. Crookes even calculated that electricity generated from coal could be used to produce sodium nitrate at £26 per ton and that power from Niagara would reduce the cost to £5 per ton. The figure for Chilean nitrate was £7 10*s.* per ton, estimates which have only relative interest at the present time. If the Haber synthetic ammonia process had not offered a much better alternative, it is possible that Crookes's idea would have been commercially developed. In any event, his thoughts were along the right lines.

Crookes was able to report that on May 10, 1897, Dewar had written to him: "This evening I have succeeded in liquefying both hydrogen and helium." Further, he mentioned that in the preceding few months, Ramsay and Travers (a distinguished Bristolian) had discovered three new rare gases of the atmosphere.

He goes on to speak of X-rays and the early discoveries of radioactivity by the Curies and Becquerel. Speculating on the source of the energy unceasingly radiated, he found it in the translational motion of the molecules and of the atoms within the molecules. He was thus led to the considerable under-estimate that the air in a room of moderate size "contains energy enough to propel a one-horse engine for more than twelve hours", and continues, "The store drawn upon naturally by uranium and other heavy atoms only awaits the touch of the magic wand of Science to enable the Twentieth Century to cast into the shade the marvels of the Nineteenth." How much more true that is than Sir William Crookes imagined.

His final section on psychical research and experimental psychology was restrained but confident. He was conscious of the criticism which he was inviting but did not shrink from giving witness to his conviction that the phenomenon of telepathy, for example, was a real one and that the subject should be studied by scientists.

May I now interpolate that we should all agree with this. Telepathy, and even pre-cognition, are claimed to have been established, and surely it is extremely important to know whether the evidence

* Presidential address delivered at Bristol on August 31 and appearing in the September issue of the *Advancement of Science*.

is really good enough to justify these claims. In the first place, we need to discuss how to set to work, and only when that is quite clear to put a plan into operation. It is the first stage which is likely to be the more difficult. Perhaps it should be added that no final verdict can be anticipated. As Dr. Warren Weaver has so aptly remarked, there is no such thing as the voice of science.

The last Bristol meeting was in 1930, with Prof. F. O. Bower as president. He spoke on "Size and Form in Plants" and remarked that this ninety-ninth year of the life of the Association marked the close of the Darwinian epoch. That was intended only in relation to personal links and did not imply that Darwin's influence on science had in any way lessened. Bower naturally referred to Sir William Crookes's remarkable address of 1898 and was able to point not only to the actual production of artificial fertilizers but also to the development of new strains of wheat which enabled areas of lower rainfall to be utilized, and also shortened the period between sowing and reaping.

So far, in asking for your attention to the reverberations of the thunder of the past, I have been on solid ground; but now the situation changes as I turn to my own choice of a topic. I recall the case of the Texas politician who was said, naturally by an opponent, to have "both feet firmly planted in the air". However, even if this is thought to apply, I hope to come to earth occasionally.

One of the troubles of the modern life of scientists is that we are all too busy, and that only a small fraction of our available time and energy can be spent in practising our real vocation. Please forgive a personal reference—it is not really very boastful. I am conscious of a certain talent for tapping a test-tube; but it is a long time since I was able to indulge this bent to much purpose. Lately I have had to be content to encourage others to perform this simple operation with, it should be added, due consideration of what is placed in the test-tube, and observation of what happens.

When the secretary of the British Association, as agent of the Council, asked me for the title of this address, I was more than usually preoccupied and selected "Science and the Scientist", because I thought it clay that could be modelled into almost any desired shape.

However, there was at the back of my mind the feeling that the functions of the two are not always clearly understood and distinguished, so that there is more than a little confusion of thought in some quarters, especially in the use of the word 'science'. The subject is not at all controversial; but I will ask for your indulgence to state once more the generally accepted definition, clearly implied in common consent if not the subject of any recent manifesto.

The Broad Definition of the Term Science

Science is simply the record, enshrined in the literature, of the system of knowledge and of the conceptions correlating parts of that system.

It is fundamental that such knowledge cannot be based on dogma or authority of any kind, nor on any intuition or revelation, unless indeed it be of the Book of Nature that lies open before our eyes. We need not dwell on the processes of acquiring knowledge by observation, experiment, and inductive and deductive reasoning. The study of scientific method both in theory and practice is of great importance; but this topic requires separate treatment. Further, it is inherent in the philosophy that the record may be imperfect and the conceptions erroneous; the potential fallibility of our science is not only acknowledged but also insisted upon.

Approximation to Truth

Still there are degrees of uncertainty, and even the apparent revolution in physics that has occurred in the twentieth century has not so much superseded the older teaching as supplemented and extended it by wider generalization.

The improvement of knowledge by successive approximations is fundamental, and it is worth while to consider some illustrations.

A very simple example of the improvement of a nearly true statement of a relation is that of the Boyle–Charles law. This stated that the volume of a gas is directly proportional to the temperature and inversely proportional to the pressure. It can be expressed as $pv = RT$. This works very well for most practical purposes; but it is not quite correct, especially at higher pressures. In 1873 van der Waals pointed out that the volume (v) taken should be lessened by a quantity (b) related (but not equal to) the volume of the molecules themselves, that is, $v - b$ for v. The pressure (p) should be increased by a quantity a/v^2 which denotes the cohesion of the molecules.

With the expression,

$$\left(p + \frac{a}{v^2}\right)(v - b) = RT,$$

a much better agreement with the experimental data was obtained; but it was still not absolutely accurate and more or less successful modifications have been suggested.

The 'ideal' gas which obeys the Boyle–Charles law may perhaps be compared to the 'ideal' railway that conforms to "Bradshaw". The divergence from the ideal in the performance of a railway becomes greater when the traffic is congested, and this factor is not unlike the b of van der Waals. But railways are much worse than gases, chiefly due to the appearance of the human factor, which is quite incalculable.

An Example to Illustrate Limits of Knowledge

Where the material can be directly apprehended by our senses, aided perhaps by devices such as cameras, microscopes and telescopes, there can be little doubt as to the validity of the record; though it may need some adjustment of errors of observation and of the value of any magnitude that is involved. The uncertainties are then largely concentrated on the conceptions and generalizations which have been based on the ascertained facts. May I be excused if I refer to the subject I know best in illustration. The molecular structures of the chemist are not directly perceptible, but must be deduced from observations on collections of very large numbers of molecules; and yet a most remarkable system has been evolved which even forms the conventional basis of patents and is recognized as valid in courts of law. The degree of uncertainty is indeed not much increased by the fact that we cannot see these structures.

Leaving aside philosophical dubieties about reality, we can say that the limits of our knowledge in this field are somewhat as follows. That the atoms exist

as entities, but the problem of the nature and constitution of the atoms themselves has not been fully solved; rapid progress is being made. We know the arrangement of the atoms in the vast majority of the species of molecules which we find in Nature, and we have learned how to construct molecules, even those of a highly complex pattern. The defences of some of the last strongholds are in process of being breached. A brilliant example of modern work of this kind is being described by the president of Section B (Chemistry), Sir Alexander Todd. It concerns the structure and synthesis of the nucleosides, those vastly important constituents of the nuclei of living cells. Even the proteins are being forced to reveal their secrets, and though none of them has yet been synthesized, we have a foretaste of what is in store in the artificial preparation of the oxytocic factor of the pituitary by Prof. V. du Vigneaud. And when we speak as chemists of nucleosides, proteins, or the oxytocic factor, we are all the time thinking not so much of the specimen in the glass tube but of the way the atoms are joined together in each of the billions upon billions of molecules in the tiniest visible fragment of the material. May I be pardoned by the *cognoscenti*, I cannot forbear from repeating the results of a calculation made by Dr. Harlow Shapley, the eminent American astronomer, which illustrates very clearly the huge number of the molecules in any small parcel of matter. In any volume of air four-fifths of the particles are molecules of nitrogen, each consisting of two atoms. If, for example, a deep breath is exhaled, the nitrogen molecules in it are rapidly dispersed and, after a certain period, probably not more than a few years, perhaps less, they will be equally distributed throughout the atmosphere. The mass of the atmosphere is known approximately, and when even distribution has been achieved, each unit volume of air at the surface of the earth will contain between two and three of the nitrogen molecules that were present in the original volume distributed. Now to make it more dramatic we select some particular exhalation, preferably a long time back so as to make sure of the necessary even diffusion. Thus we could say that when Shakespeare was writing the first line of the second act of *Hamlet*, he exhaled a breath; and if you care to inhale a breath now, it will contain two of the actual molecules exhaled by Shakespeare at that particular time. You would be very unlucky if you only got one and, if you were to capture three, you would belong to a fortunate minority.

The calculation leads to other interesting results. If you exhale a breath now, then, after the lapse of the necessary time, every breath inhaled by every living person on all occasions will contain two of the nitrogen molecules in that breath of yours.

It must be obvious that many of the molecules will be used more than once, and the calculation takes no account of the small losses from the atmosphere due to escape, or the activities of Sir Alexander Fleck, among other nitrifying organisms.

The question which is often asked, and ought to be asked, is: How can you possibly know anything about the architecture of such exceedingly minute particles? The answer cannot be given in a few sentences but an analogy may help you to appreciate the possibility—again with apologies to those *cognoscenti*. A pure chemical substance is a collection of identical molecules. Let us compare a perceptible amount of it to a crate of identical clocks; each clock then represents a molecule. We reduce the clocks to a heap of metal filings, take a certain weight of these and apply a strong magnet. This takes out the iron, which we weigh, and thus we get the percentage of iron in the whole of the clocks, which is also the percentage of iron in one clock. But it may be objected, you have started with the hypothesis that the molecules in a parcel of a pure substance are identical. How can you be sure of that? A short answer cannot be given; it depends on the recognition that certain properties, for example, many optical properties, are due to the summed effects of individual molecules, and on the elaboration of techniques for segregating molecules of the same kind; for example, by crystallization, distillation, diffusion, or by adsorption on surfaces.

Predictions and Conveyances

Justification of scientific theory has been obtained by the success of predictions and by the unanticipated convergence of more than one line of approach. This is a commonplace of every part of science, and the examples to be mentioned could be almost indefinitely extended. In all the cases the degree of coincidence becomes so great as to carry conviction that we have been vouchsafed, if not the whole truth, at least something very closely related to it.

Even the routine predictions of astronomers are a perpetual source of wonder; but the circumstances of the discovery of Neptune were sensational. In 1843–44, J. C. Adams, of Cambridge, found that the perturbations of the orbit of Uranus could be fully explained by the existence of an outer planet and he accurately identified the position in which it should be found.

Quite independently, Le Verrier, of Paris, came to exactly the same conclusions and was more fortunate than Adams in securing the quick co-operation of practical astronomers, with the result that Neptune was discovered in the predicted place in the heavens. It proved to be of the size expected and later was found to follow the expected orbit.

Again, near the beginning of the second decade of the century, Albert Einstein stated that his relativity theory required that the rays of light should be bent under the influence of a gravitational field. He was able to calculate the magnitude of the expected effect. Confirmation came from observations made on the occasions of the two next following total eclipses of the sun. This success was naturally particularly welcome in view of the novelty of the theoretical background, and it doubtless accelerated the development of the new physics based on the work of Planck, Einstein and other pioneers.

The death of Einstein has evoked universal tributes of homage to the man and the philosopher. He was undoubtedly one of the greatest of men of science.

A third example of prediction is the successful release of nuclear energy, especially in the so-called hydrogen bomb. We must not allow the horror of the possible misuse of the weapon to blind us to the spectacular triumph of the achievement. It showed very plainly that nuclear physicists had been thinking on the right theoretical lines. Let us hope that they will continue to do so—100 per cent and not 99·9 per cent. These matters are shrouded in secrecy; but Press reports have suggested that just as the hydrogen bomb is initiated by a Mark-I atomic explosion, so the hydrogen bomb in its turn can initiate a vastly greater uranium explosion. In biological processes energy is taken up in steps by suitable relays to high

levels, and an ordinary scientist cannot help wondering whether every possibility of setting fire to the lighter elements has quite certainly been excluded, if nuclear relay is taken to further stages.

Do we really know enough about nuclear reactions to be sure there is no loophole? Has every conceivable case been considered?

The physicists appear to be cocksure that this is so; they claim the 100 per cent. But just as it is necessary that 'justice shall be seen to have been done', I submit that it is desirable that the inner circle of advisers to Governments should not only be right but also be 'seen to be right'. This can only be secured by a relaxation of secrecy on the scientific aspects of nuclear energy developments, and by some form of international consultation before novel and greatly increased releases of energy are attempted.

The second class of welcome confirmations was the illumination of a topic from two or more distinct sources.

This is now a commonplace of many sciences, for example, again, of organic chemistry. Analytical researches lead to the determination of a molecular structure which may be confirmed, possibly after the lapse of years and in a different laboratory, by synthesis of the substance the molecules of which should possess that structure. If the synthetic product proves to be identical with that isolated from natural sources, the whole system of reasoning both on the analytical and synthetic sides receives important support. Even more striking has been the validation of the conclusions of chemists by X-ray crystallography. At first this led to the confirmation of established structures, often with some additional information about the arrangement of the atoms in space. Later the X-ray studies were accessory to the first analytical studies. Thus in the case of penicillin, Mrs. Hodgkin and her collaborators were able to fasten upon one of the two alternative structures which the purely chemical researches indicated. Now the chemical and physical methods go hand in hand, with the crystal analysis tending to take a more important role than in the past, and destined to do so more and more in the future.

Another example may be mentioned. The chemical formula for a metal phthalocyanine had the shape of a four-leaved clover and the metal atom was placed at the centre. J. M. Robertson's electron-distribution diagram from X-ray evidence had the same discoid shape and the metal atom was where expected. Only a few years ago a special electron microscope cast the clover-leaf shadow of a single molecule of copper phthalocyanine on a fluorescent screen. The necessary apparatus is not particularly complicated and can be purchased.

This was by no means the first time that a single molecule had become apparent to our senses. Owing to the great energy of certain particles carrying an electrical charge, and the invention of devices for amplifying very small currents, we have long been familiar with a variety of methods for observation of the effects of single particles: Crookes's spinthariscope, the Geiger counter, the Wilson cloud chamber and—bringing coals to Newcastle—the techniques developed in such an admirable manner by Prof. C. F. Powell and his school in Bristol, for the study of single events at the atomic level—the study of the impact of cosmic rays on the sensitive emulsions of the photographic plate.

In other subjects there are very many examples of the confluence of streams springing from two sources. Examples are the dating of ancient objects by means of radioactive isotopes and the light shed on the movements of races by the hybridization of plants.

The latter reference is to a hybrid cotton plant found in Peru. There the Porto Rican cotton plant married the Indian, and the circumstances are consistent with the view that Indian voyagers reached South America in very early days.

Scope of Science

All branches of knowledge are included in the definition of science; always provided that the record has been made without appeal to authority. There is no philosophical line of demarcation that separates one branch of learning from another, though the demeanour of many so-called humanists suggests that they perceive one.

A historian, formerly a colleague of mine, once declared that the absorbing interest of his subject was due to the badness of the evidence; but I think that was only another way of saying that he liked to get his teeth into a difficult problem.

It is not necessary or desirable to pursue this unprofitable discussion; our brother scientists on the Arts side will doubtless work out their own salvation. But it is opportune to add another voice of protest against the often repeated assertion, that 'culture' is the preserve of students of some particular branches of knowledge, and is denied to others. What is meant by 'culture'? If it is the ability to think clearly and to express thoughts in language, what evidence is there that physical and biological scientists lack these qualities?

The acerbity that characterizes much of the anti-scientific propaganda strongly suggests that its basis is defensive rather than offensive.

In the meantime, these persistent attacks do a great deal of harm by suggesting to young people that they will become automatons if they devote themselves to physical science and that in such a life they will not experience the warmth of human feeling.

The Joy of Discovery

What a travesty of the facts this is! The scientific discipline is indeed a hard one; but it is most rewarding and full of living drama. Perhaps I may just have time to show one picture from the past. It concerns a beginner in research, but that young man was Louis Pasteur. It will be best to quote his own words from "Lectures on the Molecular Asymmetry of Natural Organic Products" (1860), reprinted by the Alembic Club of Edinburgh:

"When I began to devote myself to special work, I sought to strengthen myself in the knowledge of crystals, foreseeing the help that I should draw from this in my chemical researches. It seemed to me to be the simplest course, to take, as a guide, some rather extensive work on crystalline forms; to repeat all the measurements, and to compare my determinations with those of the author. In 1841, M. de la Provostaye, whose accuracy is well known, had published a beautiful piece of work on the crystalline forms of tartaric and paratartaric acids and of their salts. I made a study of this memoir. I crystallized tartaric acid and its salts, and investigated the forms of the crystals. But, as the work proceeded, I noticed that a very interesting fact had escaped the learned physicist. All the tartrates which

I examined gave undoubted evidence of hemihedral faces.

"Now if we compare the disposition of the hemihedral faces on all the prisms of the primitive forms of the tartrates, when they are orientated in the same manner, this disposition is found to be the same."

Herschel had already connected the hemihedry of quartz with the right or left rotatory power of the crystals, and Pasteur saw that the hemihedry of the tartrates could be an outward and visible sign of the asymmetry of the molecules. Hence he was extremely interested in a note of Mitscherlich of 1844 stating that sodium ammonium tartrate and sodium ammonium paratartrate had identical crystal form and yet only the former was optically active in solution. He thought Mitscherlich must have failed to notice that the paratartrate was not hemihedral.

"I hastened therefore to re-investigate the crystalline form of Mitscherlich's two salts. I found, as a matter of fact, that the tartrate was hemihedral, like all the other tartrates which I had previously studied, but, strange to say, the paratartrate was hemihedral also. Only, the hemihedral faces which in the tartrate were all turned the same way were, in the paratartrate inclined sometimes to the right and sometimes to the left. In spite of the unexpected character of this result, I continued to follow up my idea. I carefully separated the crystals which were hemihedral to the right from those hemihedral to the left, and examined their solutions separately in the polarizing apparatus. I then saw with no less surprise than pleasure that the crystals hemihedral to the right deviated the plane of polarization to the right, and that those hemihedral to the left deviated it to the left; and when I took an equal weight of each of the two kinds of crystals, the mixed solution was indifferent towards the light in consequence of the neutralization of the two equal and opposite individual derivations.

"The announcement of the above facts naturally placed me in communication with Biot, who was not without doubts regarding their accuracy. Being charged with giving an account of them to the Academy, he made me come to him and repeat before his eyes the decisive experiment. He handed over to me some paratartaric acid which he had himself previously studied with particular care, and which he had found to be perfectly indifferent to polarized light. I prepared the double salt in his presence, with soda and ammonia which he had likewise desired to provide. The liquid was set aside for slow evaporation in one of his rooms. When it had furnished about 39 to 40 grams of crystals, he asked me to call at the Collège de France in order to collect them and isolate before him, by recognition of their crystallographic character, the right and the left crystals, requesting me to state once more whether I really affirmed that the crystals which I should place at his right would deviate to the right, and the others to the left. This done, he told me that he would undertake the rest. He prepared the solutions with carefully measured quantities, and when ready to examine them in the polarizing apparatus, he once more invited me to come into his room. He first placed in the apparatus the more interesting solution, that which ought to deviate to the left. Without even making a measurement, he saw by the appearance of the tints of the two images, ordinary and extraordinary, in the analyser, that there was a strong deviation to the left. Then, very visibly affected, the illustrious old man took me by the arm and said :—

" 'My dear child, I have loved science so much throughout my life that this makes my heart throb.'

"You will pardon me, gentlemen, these personal recollections which have never been effaced from my mind. In our day, and with our habits, they would offend in a scientific memoir, but they have seemed to me not out of place in an oral account; and perhaps the biological interest of such recollections will constitute one of the advantages of the kind of instruction which the Société Chimique inaugurates to-day.

"Indeed there is more here than personal reminiscences. In Biot's case the emotion of the scientific man was mingled with the personal pleasure of seeing his conjectures realised. For more than thirty years Biot had striven in vain to induce chemists to share his conviction that the study of rotatory polarization offered one of the surest means of gaining a knowledge of the molecular constitution of substances."

Thus was the science of stereochemistry born. The main point of Pasteur's work was that he recognized that the optical rotatory power of the tartrates in *solution* showed that the molecules themselves must be asymmetric. The hemihedry of quartz was recognized as due to the asymmetrical arrangement of symmetrical molecules in the crystal, like a right- or a left-handed screw. But the hemihedry of the tartrates, though due in the crystal to the same cause, persisted in the free molecules, which were therefore characterized by asymmetry in a certain sense, all right-handed or all left-handed. A mechanical analogy is very useful here. If a pipe is almost filled with one screw and water is forced along it, the issuing stream will rotate, clockwise or anticlockwise, according to the nature of the screw. The same thing happens if we pack the tube with small right-handed screws, or left-handed screws, arranged at random. With an equal number of both kinds there would be no rotation of the issuing water. The single large screw corresponds to the quartz crystal, and the higgledy-piggledy small screws correspond to the freely moving asymmetrical molecules of the tartrate in solution.

The philosophy of asymmetry is very intriguing. Only an asymmetric, I should perhaps say dissymmetric, agent can distinguish the right-hand from the left-hand varieties, and of course it was Pasteur himself who sorted out the hemihedral crystals oriented in the two mirror-image forms.

Recently, by a most ingenious application of crystal analysis, Prof. J. M. Bijvoet has been able to determine the configuration of naturally occurring tartaric acid, which is related to that of many other substances in what we call the L-glucose family. The convention used by stereochemists turned out to be correct, which is quite convenient. The chances for this outcome were naturally no better than even.

Admittedly this example of 'atmosphere' relates to a very exceptional occasion and Pasteur was a very remarkable man, one of the greatest scientists of all time.

But all of us who have been privileged to take part in scientific life and research have had similar exciting and may I add, emotional, experiences in our smaller ways.

The explanation of molecular asymmetry in terms of structure was given in 1875 by Le Bel and independently by van 't Hoff. The latter was said by Kolbe "to have no taste for exact science. Mounted

on his Pegasus, evidently borrowed from the veterinary stables (van 't Hoff taught chemistry at the College of Veterinary Science at Utrecht) he thinks he can see the arrangement of the atoms in space". Nowadays the organic chemist has not completed a synthetic task until the various parts of the molecule have the desired type of asymmetry. Often a succession of screws has to be laid down in the correct order. Thus there are eight places in the cholesterol molecule which can be the source of asymmetric arrangement and that implies 256 possibilities of difference due only to the right- or left-handedness of the parts. Yet the one particular configuration of the cholesterol molecule has been constructed in the laboratory.

The task was equivalent to finding the right road at each of eight successive bifurcations.

Before straying too far from actual science I would like to say something about the use of speculation, which should always arise from some correlation which was hitherto unknown or unnoticed, or the significance of which has been under-estimated, or thought to have been under-estimated.

In the early stages it may be discouraging to study the literature; one can always find reasons there for believing that a new idea is not worth pursuing, or for that matter for saving oneself the trouble of doing an experiment. Later, having worked up an internal, self-generated pressure of enthusiasm, it is obligatory to face the hard facts already on record. Then, instead of weakly accepting a rebuff we shall at least put up some fight with 'but on the other hand it might be that——'.

At this point it may be realized that something really has been overlooked by one's predecessors, and new crucial experiments must be devised to resolve one dubiety after another. Herein lies the safeguard, the final court of appeal is Nature herself.

The scope of the British Association's interest is not clearly defined in our original "Objects" which I will read, because the early practice of printing them each year has long been abandoned.

"The Association contemplates no interference with the ground occupied by other institutions. Its objects are:—To give a stronger impulse and more systematic direction to scientific inquiry,—to promote the intercourse of those who cultivate Science in different parts of the British Empire, with one another and with foreign philosophers,—to obtain a more general attention to the objects of Science, and a removal of any disadvantages of a public kind which impede its progress."

Our sections now include the chief physical and biological sciences, economics, anthropology and archæology, psychology, and education, as well as engineering, forestry and agriculture. There is also an active Division for the Social and International Relations of Science. All this evinces a real catholicity of interest; omissions are based on practical, rather than on theoretical considerations. For example, clinical medicine and surgery are well served by a number of other organizations.

The Universality of Science

An important aspect of science is its universality and its fully international character only rendered possible by the axiomatic freedom it enjoys from authority and dogma. True there have been regrettable incidents such as that of the Michurinian genetics, but the body scientific has acquired considerable immunity against such fevers, which are now all but forgotten.

There is no such thing as British, or German, or Russian, or Jewish science, though it is true that the genius of many peoples has enabled them to make characteristic contributions. The French have a special lucidity of mind and expression, as well as elegance of technique, the Americans excel in organization of research, the Germans in thoroughness, the Japanese in attention to detail, while the Jews have imagination and theoretical insight. There is much overlapping of course; we may find a Memling among the Florentines.

It has occurred to me to wonder wherein lies the particular merit of the British. The pioneering spirit of our illustrious forefathers is everywhere acclaimed; and there is even now little sign of any decline in our initiative. Can it be at least partly the result of not working so hard that no time is left for reflexion? We certainly defend our leisure from encroachments with special tenacity. Again, it may be that we are in a certain sense undisciplined; but I will not elaborate that suggestion.

Science as a common international possession and the world-wide co-operation in its development must be bulwarks of peace. Here we have a realizable asset of the greatest possible positive value. The brotherhood of scientists is a reality. It is fortunately not unique; there is a similar fraternity among sportsmen, musicians, chess-players, and other categories of enthusiasts, not encumbered by the so-called ideologies.

It should be clear from the definition that religion, in so far as it is authoritarian and dogmatic, can make no contribution to science. But it is no paradox to say that there is room for both science and religion within the mind of a man. Indeed we have ample proof that this is so from everyday experience. Many of the greatest of scientists have been deeply religious men and eminent divines have been excellent scientists. Testimony to the help afforded to the Royal Society and to the British Association by churchmen may be found in the records and is still apparent on inspection.

At the Bristol meeting of 1836 the presidents of Sections A, B, C and D were all clergymen.

On the other hand, many scientists have been, and are, free-thinkers. Yet, the two groups should be united in the unflinching pursuit of the truth and in a spirit of tolerance.

Titles such as "Science and Religion" tend to confuse the simple issue which relates to "Scientists" and not to "Science". Again, the uncertainties of modern physics are now considered by some to justify mystical analogies.

Models and Pictures replaced by Equations

It would appear that there are many who feel an irresistible urge to fill up all gaps in knowledge by their own inventions. Such intuitive speculation can indicate useful working hypotheses, but it is not science *per se*.

Actually physics is a much more complete and satisfactory system now than it was in the later nineteenth century. It is true that we are perplexed by many new problems; but at least we are aware that they exist. That is an improvement on complacency founded on ignorance. The human mind can be shown to be inadequate in several ways. Speaking subject to correction by philosophers, it would seem that certain alternatives that are logically mutually

exclusive are equally inconceivable by us. The conceptions of a continuum or a discontinuum, of creation or indefinite projection of reality into the past, cannot be grasped by the human mind. Yet the fact that we find something to be incredible is no proof that it does not exist. Hence physicists are content to derive expressions for the description of phenomena with the minimum of extraneous hypothesis.

Fifty years ago an ether was an article of faith; there could be no action at a distance without an intervening link, electromagnetic waves must occur in something, and so on. But each invented ether was about as incredible as the absence of one. Nearly fifty years ago I remember being fascinated by Osborne Reynolds's "Inversion of Ideas as to the Structure of the Universe". This postulated a very dense ether consisting of minute grains. Matter was a negative inequality in this ether, that is, an absence of grains, a kind of hole. It could be shown that the inverse-square law would govern the attraction of such holes. Mathematical physicists have evidently found it hard or impossible to use this theory, but I well remember thinking more or less as follows: Yes, but what are the grains made of? They have been invested with some of the properties of matter and they are a kind of engineer's dream of perfect Daltonian atoms on a very small scale. The argument is quite inconclusive; but I found then, and have always found since, that the idea of a continuum, even inside a grain of the ether was, and is, repugnant. So nowadays we just arrange to do without this comfortable resource of an ether, though I have been told that Prof. P. A. M. Dirac may revive the idea in some form or another—which few of us will understand.

Scientists' Duty to the State

Another feature of the complex make-up of the scientist is that he is a citizen and owes a duty to the State. This leads to obvious conclusions in most respects. But where do we stand in regard to the invention of military weapons?

In war-time few of us entertained any doubts about this; it was our clear duty to help our country to the best of our ability. Now it is a matter for the conscience of the individual, taking into consideration that we have not yet achieved a real peace.

The position is one of unstable equilibrium, and it still seems that as citizens we should do all in our power to strengthen the hands of our leaders, in whom we have placed our trust, and who we believe are sincere in their desire to achieve universal peace among the nations. Even the terrible nuclear-energy weapons must be developed by our scientists in peace-time so as to build up the necessary potential of deterrence, our only defence against extinction.

Scientific, Technical and Technological Man-Power

I have mentioned only two or three of the personal problems of scientists; but there are, as is very well known, many national questions affecting scientists, and on these I can say very little in this address. The chief is perhaps the training of scientists, which starts in the schools and involves the supply of science teachers. This matter has been raised in the General Committee of the British Association here in Bristol. We hope to make some small contribution to a solution of difficulties that are the cause of considerable justified anxiety among industrialists.

Finally, I want to say a word about technology and technologists. Dr. E. J. Holmyard and Prof. Charles Singer have, in the first volume of their monumental work on the "History of Technology", pointed out that it all started with handicraft. The modern technologist is still a man with 'know-how' and 'can-do', whether he be technician, craftsman, designer or supervisor, and he is equipped with knowledge of general and special scientific and engineering subjects.

It is generally agreed that we have far too few technologists in Britain, and that our economic survival is improbable unless we take some drastic steps to remedy the situation. The proposals that have been made are admirable so far as they go; but, in the opinion of many who are well qualified to judge, they do not go nearly far enough.

It is doubtful whether any scheme tied to existing institutions can meet the requirements; and to come right down to ground-level, I suggest that we need a 'Technology Grants Committee', independent of any other body and with power to expend a very considerable sum of money. This idea has probably already been adumbrated. In which case I think it should be supported.

SUMMARIES OF ADDRESSES OF PRESIDENTS OF SECTIONS

VACANT LATTICE SITES IN CRYSTALLINE SOLIDS

THE physics of the solid state is a wide field which ranges from abstract subjects rather remote from practical applications, such as superconductivity, to problems of immediate technical importance such as the development of crystal valves and the behaviour of the materials of a reactor under heavy bombardment by neutrons. In his presidential address to Section A (Mathematics and Physics), Prof. N. F. Mott concentrates on one theme—the existence and behaviour of vacant lattice sites in crystalline solids—and shows its importance for a wide range of phenomena. A vacant lattice site is a point in a crystal lattice from which the atom or molecule, which ought to be present in the crystal structure concerned, is in fact absent. Vacant lattice sites occur in a crystal in thermodynamical equilibrium, some of the atoms having moved away as a result of the influence of heat motion. They can also be formed as a result of bombardment by fast particles and also by subjecting the metal to cold work.

The first importance of the concept of a vacant lattice site is its role in explaining the phenomenon of diffusion in solids and particularly in metals. When two metals are welded together and a region intermediate between them is formed which is occupied by an alloy of the two, diffusion has taken place. This means that two adjacent atoms in a crystalline solid are able to change places if the temperature is high enough. It is now believed that atoms change places only with the help of a vacant lattice site; a simultaneous movement of two atoms would involve a very high energy and is improbable,

but the loosening of a crystalline lattice near a vacant lattice site greatly lowers the energy necessary.

We are interested, therefore, in measurements of the number of vacant lattice sites in a solid and the rapidity with which they change places. One method of obtaining this information is by the use of radioactive tracers which enables the coefficient of self-diffusion to be determined. Another method which has come to the fore recently is by the use of the 'nuclear spin'—the magnetic moment carried by the nucleus of the atom concerned. When the substance is placed in a magnetic field, the existence of quantized stationary states associated with the nuclear spin in the field enables substances to absorb electromagnetic radiation of radio frequencies. The width of the absorption line depends on the extent to which each nucleus interacts with its neighbours and will vary from one nucleus to another according to the orientation of the magnetic moments of the adjacent nuclei. If, however, the atoms are changing places rapidly, this interaction between nuclei will be averaged out, and the breadth of the absorption line will be greatly decreased. In this way, it is possible to estimate the jump frequency.

Two methods, therefore, both borrowed from nuclear physics, are available for measuring the frequency with which atoms change places. The evidence that they change places with the help of vacancies comes from many sources, of which perhaps the most important is the fact that diffusion in solids is greatly accelerated after they have been bombarded by fast charged particles. It is thought that the vacancies produced by this bombardment remain in existence for a considerable time and, as they move about throughout the crystal, enable atoms to change places much more rapidly and at lower temperatures than would be possible without their presence. It is estimated that one fast neutron will displace as many as six thousand aluminium atoms before it is brought to rest, and one diffusion fragment in uranium as many as twenty-five thousand uranium atoms, so the effect need not be small. As a result of these displacements, the phenomenon known as radiation damage occurs in solids exposed to the radiations in a reactor. Mechanical strength, density and electrical resistance can all be affected. The subject of radiation damage is under active investigation at the present time, and there is still much which we do not understand about it.

Finally, it seems very likely that vacancies play a part in the phenomenon of metallic fatigue. As already mentioned, deformation or cold work is known to produce vacancies in a solid, with effects on its properties in many ways similar to those which result from nuclear bombardment. In fatigue one knows that localized regions of the solid, which show up as visible slip bands, are deformed plastically backwards and forwards many millions of times. Both experiment and modern theoretical concepts lead one to believe that in these regions very large numbers of displaced atoms are formed, so that after a time the crystalline structure of the molecule will be broken down as it would under intense bombardment. A detailed theoretical explanation of fatigue in terms of atomic movements has not yet been formulated; but one very possible view is that this break-up of the crystalline structure and consequent recrystallization would lead to intense local strains, and that it is these strains, perhaps accompanied by the effect of oxygen or other gases, which initiate a fatigue crack.

NUCLEIC ACID STRUCTURE AND FUNCTION

SIR ALEXANDER TODD points out in his address to Section B (Chemistry) that, although the nucleic acids rank with the proteins and carbohydrates as one of the three major groups of macromolecular substances of vital importance in living matter, yet until recently they were the subject of very little detailed chemical investigation. This may have been due, in part, to the lack of suitable experimental techniques for dealing with them, but it was also due to a surprising lack of chemical study of the simple nucleosides and nucleotides from which they are built up and which can be obtained from them by hydrolysis. All this is now changed, and we have seen in recent years a veritable revolution which has led to a satisfactory formulation of the nucleic acids and a much clearer understanding of their chemical behaviour. This in turn has opened the way to fresh advances in biochemical and biological studies which have brought us closer to an understanding of their function.

Two types of nucleic acid occur in Nature—the ribonucleic acids and the deoxyribonucleic acids. Graded hydrolysis of them yields first the nucleotides, which are phosphates of the nucleosides, the latter being N-glycosides of certain purine and pyrimidine derivatives. The essential difference between the two types is that the sugar involved in the ribonucleosides is D-ribose, and in the deoxyribonucleosides, 2-deoxy-D-ribose. Both types of nucleic acid can be regarded as polynucleotides, the individual nucleotides being linked one to the other by phosphodiester linkages. Based on detailed chemical studies which have clarified the structure and chemistry of the nucleosides and nucleotides, a general theory of the structure of nucleic acids as 3′ : 5′-linked polynucleotides has been developed. All subsequent work appears to confirm this theory, which has become generally accepted. Synthetic work in the polynucleotide field is still in its early stages; but dinucleotides of known structure have been synthesized, and their properties are in accord with the general theory of nucleic acids. Further work is also needed in connexion with the determination of the sequence of nucleoside residues in individual nucleic acids, since it is on this sequence that much of their biological specificity must depend.

Since the emergence of a clear structural picture of the nucleic acids, much attention has been paid to their macromolecular conformation. In this field, X-ray examination of deoxyribonucleic acids has been a fruitful source of information. According to current views, deoxyribonucleic acids appear to exist in the form of two opposed helical polynucleotide chains wound together in a double helix and held together by specific hydrogen-bonding between the purine and pyrimidine bases occurring in each nucleoside residue. This picture of deoxyribonucleic acid existing in a double helical structure has much in its favour and is now generally accepted in all essentials. One of its most interesting features is the indication it gives of how a deoxyribonucleic acid may represent a self-duplicating system. This is particularly important, since deoxyribonucleic acids are constituents of the chromosomes, and there is evidence to suggest that in bacteria they are the actual genic material which determines hereditary transmission. This concept of the nucleic acids as replicating systems is also of great interest in con-

nexion with viruses and bacteriophages, all of which contain nucleoproteins. There has, too, been considerable speculation regarding the way in which nucleic acids may be involved in protein synthesis; it is possible to suggest a mechanism for protein synthesis by means of nucleic acid, but no evidence is as yet available to show whether it has any validity. Clearly, however, we are now at the opening of a new phase in the study of nucleic acid function, and future developments are likely to have profound repercussions in biology and medicine.

GEOGRAPHICAL DISTRIBUTION OF PAST FLORAS

BIOGEOGRAPHY in relation to geological history was the starting-point of Darwin's evolutionary studies, and the opening sentence of the introduction to the "Origin of Species" reads: "When on board H.M.S. Beagle as naturalist I was much struck with certain facts in the distribution of the organic beings inhabiting South America, and in the geological relations of the present to the past inhabitants of that continent". Thus, in his address to Section C (Geology), Mr. W. N. Edwards shows that the biogeography of the past is still worth the attention of the student. Earth-movements and erosion, mountain building and climatic change are all reflected in the altered distribution of floras and faunas over the world, and the past distribution of animals and plants is often cited in support of the conclusions reached, or the theories put forward, by geologists and geophysicists. It is therefore well occasionally to review our knowledge of the relations between faunas and floras on one hand, and continents, seas and climates on the other.

Former land and sea areas can be roughly mapped by plotting occurrences of terrestrial and marine fossils, and geological evidence of past climates can be supplemented from the biological side. Land plants, in spite of arguments adduced to the contrary, are particularly valuable in this respect, especially if one considers ecological types of vegetation rather than individual plants. A review of fossil floras from the Devonian onwards in relation to known geological changes suggests that, although in the Devonian, and to a lesser degree in the Jurassic, world vegetation was fairly uniform, latitudinal zonation is more normal. The supposed uniformity of Jurassic floras is partly illusory, and there are in fact clear indications of zonation in that period; the early Devonian floras are not yet sufficiently well known ecologically for one to say whether they were so uniform as they appear to be. The plant evidence further indicates that since the late Palæozoic there have been vegetational and climatic zones parallel to the present equator. Deviations from exact parallelism, such as the arid belt running from eastern Asia across north Algeria, are due to the past conformation of the land masses of the northern hemisphere being similar to that of the present day. The vegetational succession in the northern hemisphere does not, therefore, lend support to notions of extensive wandering of either continents or poles, at any rate since the Carboniferous. Following the orogeny and glaciation of the upper Carboniferous and Permian, the major influences on plant distribution in the Old World have been the arid belt just mentioned and the Tethys Sea. The latter, which was in existence from the upper Palæozoic to the middle of the Tertiary, must have formed an important migration route for plants, not only along its shores but also by marine currents. Transmarine distribution of land plants would have been particularly easy in a sea which ran for some six thousand miles from east to west in an equable climatic belt of much the same character throughout. Angiosperms may possibly have arisen in the Indo–Malayan region and spread first along the Tethys Sea.

IMMUNITY AND DEVELOPMENT

THE borderland between immunology and embryology, which forms the subject of the presidential address by Prof. F. W. Rogers Brambell to Section D (Zoology), has proved a fruitful field of research in recent years. The relationship of mother and young in mammals involves special immunological problems, for reactions resulting from immunological incompatibility can cause severe damage or death of the fœtuses, perhaps of the mother also. An adult mammal responds immunologically to the introduction into its system of tissues, cells or proteins from another species, or even from another individual of the same species. These reactions are an expression of the individuality of the adult organism.

The capacity to respond immunologically to the introduction of antigens into the system by the production of circulating antibodies appears relatively late in development. Circulating antibodies cannot be produced in detectable amounts by active immunization of new-born animals. The mammalian fœtus and the chick embryo can tolerate exchange of blood-forming cells of different blood group from a non-identical twin, and become permanent blood-group chimæras. Grafts, even of mammalian tissues, can be grown on the chorio-allantoic membrane of the chick embryo, without apparent reaction.

At a still earlier stage of development, the zygote nucleus comes into being in an environment of maternal protein. The cytoplasm of the egg must contain maternal proteins that would be antigenic in the adult offspring, yet it is in this medium that the zygote nucleus must function and initiate the synthesis of all those proteins which will characterize the new organism. Hence the fœtus and the young animal are tolerant of foreign proteins, cells and tissues to which they would react immunologically when adult. This immunological inactivity obviates the risk of the fœtus reacting to incompatible maternal antigens during pregnancy, but leaves it unable to protect itself against infection at birth. Temporary protection is afforded by the passive transference of antibodies from the mother.

These considerations lead to a discussion of the route by which passive immunity is transferred from mother to young, of hæmolytic disease of the new-born, of the selective passage of proteins through the entoderm and of experimental induction of tolerance to antigens. Most of the topics concern recent work which appears to advance both mammalian embryology and immunity. These studies have thrown new light on the functions of the fœtal membranes in mammals and on the causes of much pre-natal and neo-natal mortality. They have shown that the cells of the fœtal membranes can selectively transport globulin according to the species in which it was synthesized. This selectivity has some resemblance to other known immune responses, such

as passive anaphylactic sensitization of smooth muscle; but the mechanism by which it is effected remains to be elucidated. It is exercised at a much earlier stage of development than that at which the organism can produce circulating antibodies. Likewise, in actively acquired tolerance, we have evidence of the embryonic cells being permanently modified by exposure to specific antigens at a stage long before they are capable of reacting by the production of antibodies. Why exposure of embryonic cells to antigen should inhibit the subsequent formation of the specific antibody by these cells, whereas exposure for the first time of adult cells to the same antigen induces the formation of the specific antibody, is another problem that awaits solution. The solution of these problems may be expected to give us a glimpse, at least, of the fundamental cell processes involved in the fabrication of specific proteins. All these phenomena may depend on the 'recognition' of the molecular form of proteins by the cells, a property which might prove to be of wide significance in embryonic differentiation.

Immunological methods open the way for experimental approaches to problems of vertebrate embryology that hold promise of great advances in our understanding of the development of function—of physiogenesis rather than morphogenesis.

GEOGRAPHY AND ITS HISTORY

THE history of geography has never before been the subject of a presidential address to Section E (Geography); Mr. J. N. L. Baker's address to the Section this year deals exclusively with geography between 1500 and the beginning of the nineteenth century, and is especially appropriate to the city of Bristol, with its connexions with Robert Thorne the younger, Richard Hakluyt, and the merchants of Bristol who searched for the island of Brazil. He points out that the history of geography is a subject that can be approached only with the help of the work of many scholars, including Dr. R. Hartshorne, Mr. G. R. Crone, Dr. J. A. Williamson, Dr. O. J. R. Howarth and, above all, of Prof. Eva Taylor.

The growth of academic geography in Britain has been one of the remarkable facts of the past fifty years; but those who speak of the 'new' geography would do well to consider the present position of the subject, in H. R. Mill's words to Section E in 1901, as "the outcome of the thought and labours of an unbroken chain of workers". The nineteenth-century 'new plans' for geography of Pinkerton and Malte-Brun, like those of Buache and Busching of the century before, drew heavily upon older works. Geographers throughout the past four centuries, at least, have always been dealing with 'the old imperfectly composed' and 'the new lately reformed'. Geography is a subject that is always alive: without that life it is nothing.

In the past, geography has been regarded as an essential part of general education. John Milton, John Locke, Samuel Johnson and Edward Gibbon all showed clearly their appreciation of geography as part of the culture of an educated man, and the works of Bodin, Rousseau, Buffon, de Brosse and others in France suggest that the position was similar there in the seventeenth and eighteenth centuries. In the writings of the period it was assumed that readers would appreciate geographical references: many of Milton's lines, for example, mean little or nothing without a knowledge of geography as understood in his day.

The ability of geography to help in practical affairs is no new feature of the subject. It has always had a practical side, both in the narrow sense through its contributions to exploration and navigation and, in a wider manner, through its close connexion with classical studies. Annotated editions of the works of classical geographers, and bulky geographical dictionaries, were common and indicate the status of the subject. If modern geographers can make similar contributions to the general culture of our age, they will, in that way alone, justify their existence.

Although for long periods there has been geography, for equally long periods there have been no geographers in the technical sense of the word. There was no accepted meaning for the term 'cosmography', which was replaced by 'geography' in the eighteenth century. Furthermore, geography was not confined to those who wrote cosmographies and geographies. The conclusion is clear that there have been many kinds of geography and many writers of geography, so that present-day geographers should be modest and circumspect in their claims. No geographer need apologize for, or be ashamed of, his subject, with its long and honourable history that offers almost unlimited scope for research. The rewards of such research will be great, because it is only when the geography of our day is seen against the background of its history that its present position can be appreciated and its future prospects assessed.

THE ENGINEER AND MARINE TRANSPORT

IN his address to Section G (Engineering) on "The Influence of the Engineer on Sea Transport and Trade", Dr. S. F. Dorey points out that during the latter part of the eighteenth century warfare and monopolies were responsible for the British attitude of complacency towards overseas trade and commerce. The lack of competition resulted in stagnation of the design and development of merchant ships. The Americans, however, on achieving political independence, exploited the merits of cheap, fast sailing-ships and captured the bulk of the trade and traffic on the Atlantic and Pacific. Moreover, the progress they made in the design and development of the clippers gave them an almost unsurpassable lead in sail, and it was due entirely to pioneering the steamer that the British were able to offset the advantage gained by the Americans. During the 1830's, steamers commenced a trans-Atlantic service; but their use on longer sea-routes was impeded by the high coal consumption of the machinery. It was not until the 1860's, when the more economical compound engine was introduced, that impetus was given for the expansion of trade to the Pacific.

At about the same time processes for the quantity production of cheap mild steels were evolved, and construction of ships and machinery of mild steel gave rise to greatly improved performance and reliability. Economy of coal consumption improved again with the adoption of the triple-expansion steam engine and enabled steamships to open up trade with the Far East. The development of the liner demanded machinery of high power; but steam reciprocating engines proved unsuitable where more than 40,000 I.H.P. was required for a single ship. The solution

to this problem was provided by Charles Parsons at the end of the nineteenth century. His steam-turbine power plants enabled machinery of 70,000 S.H.P. to be installed in a liner in 1907, and the service coal consumption was nearly one-half that of the equivalent reciprocating machinery.

Following the First World War, when competition was keen to secure freights, the engineer played his part in reducing the operating costs of merchant ships. The increased use of oil-firing, the remarkable development of the oil engine, and greatly improved boiler and steam-engine designs may all be cited. When trade eased, the drive for still further economy was maintained, and at the same time the power outputs of machinery were increased to meet particular needs. During recent years particular attention has been given to the machinery of large single-screw tankers and ore-carriers which handle the raw materials of the expanding oil, chemical and metallurgical industries. The new types of prime movers are also receiving consideration. The marine gas-turbine is still in the development stage and has not reached the degree of long-service reliability and low fuel-consumption enjoyed by the other two main types of prime movers.

The marine application of atomic energy offers great attractions, particularly so far as weight and storage of fuel is concerned, but will only become possible by reducing to attractive levels the weight, size and cost of reactors and screening, and the satisfactory solution of control and maintenance problems. Auxiliary shipboard machinery has also played a prominent part in establishing new trades. The introduction of refrigerating machinery, for example, opened up the meat and farm-produce trade and brought prosperity to Australia, New Zealand and South America. The achievements of marine engineers have had considerable influence on the prosperity of nations and, with the aim of improving our living standard still further, the engineer will no doubt continue to play his part, as evidenced by the high rate of progress maintained during the post-war years.

ANTHROPOLOGY—A SCIENTIFIC UNITY?

DR. D. B. HARDEN, in his address to Section H (Anthropology), comes to the conclusion that the question whether anthropology is or is not a scientific unity is one that, after careful thought, can only be answered in the affirmative. He has, he says, never been answered in any other way by those who have taken part in developing anthropological studies within the British Association from its earliest years, as is clear by even a cursory glance at the historical growth of the subject up to the time when it attained sectional status in 1884 and in the years beyond.

Up to 1846 anthropology, or ethnology as it was then usually called, only appeared sporadically in the Association's programmes—usually in the Zoology or Geology Sections. In that year Section C (Geology and Geography) formed a sub-section for ethnology, and in 1851, when geography became a separate Section (E), it carried ethnology with it. The next change was in 1869, when anthropology became one of several departments within Section D (Biology). The ethnological side of Section E was then abandoned and the Geology Section ceased to carry papers on human palæontology (as it had done from time to time hitherto). This arrangement lasted until 1884, when anthropology was promoted to full sectional status as Section H.

Since then the Section, both in its annual programmes and through its research committees, has spread its net fully and widely over the whole field of anthropology—the study of man in the past and in the present. Its programmes have embodied prehistoric and historic archæology (both British and foreign), physical and social anthropology, ethnology (both of primitive and advanced cultures), folk-lore and comparative religion. The Section's interests are, indeed, broad—as broad as, if not broader than, those of any other Section. So broad are they that it is impossible for them all to be properly represented in the programme every year; and the more specialist-minded of anthropologists, ethnologists and archæologists are inclined to complain of this and to say that it is no longer worth their while to come to the meetings unless the Section is split into two or more parts, so that more papers in their own special subjects can be fitted in. This, Dr. Harden maintains, is a misguided view, for the British Association has always endeavoured to be a widely ranging body, and its objects would be defeated if one of its Sections became just another specialist conference.

Anthropology touches on so many other sciences and disciplines that there is no one sectional activity within the Association with which it does not have mutual contact at one time or another, as its previous alliances with geology, geography and biology make plain. Anthropology, the study of the human race, is, indeed, by its very nature a microcosm of the Association as a whole; properly used, the Section programme could be a unifying influence within the Association. Dr. Harden maintains that this aim cannot be achieved by trying to plan programmes which will attract and satisfy diehard specialists in even some, let alone all, of the various branches of the subject, for this will interest neither the specialist nor the more broad-minded student. He proposes that advantage be taken of the existence of other Sections in the British Association to arrange programmes which will show the overlap between them and Section H, and that members of Section H should at the same time hold fast to their unity and pay no attention to any pressure from the partitionists. To accept such pressure, he concludes, and thus encourage specialization, will do disservice not only to anthropology as a unified scientific discipline but also to the Association's own interests and policies.

THE SENSORY PATHWAY

THE pathway from receptor to brain is in all cases built up on much the same pattern and involves a series of interruptions or relay stations where the message is transferred to a new set of nerve cells. The theme of Dr. R. S. Creed's presidential address to Section I (Physiology) is the significance of these breaks in continuity on the way to the sensorium. What is the reason for their existence? Why is there not a direct route for through traffic? Satisfactory answers to these questions cannot be given by simply comparing the nervous system with a network of telephone lines and the relay stations in grey matter with telephone exchanges. As is known from studies of reflex action, much more may happen at synapses between nerve

cells or their processes than mere shunting of excitation along certain channels. Inhibition, facilitation and after-discharge may be expected to occur in the sensory, just as they do in the reflex, pathway.

Until recently, it was necessary to rely mainly on subjective human experience in work on the physiology of the senses. Now, as a result of improved electro-physiological methods introduced by Adrian, it is possible to make objective records of events in single units at various levels in the ascending sensory pathway. Such studies not only provide a valuable check on what can otherwise be only rather indirectly inferred, but also give definite information about the anatomical site at which modification of messages occurs. There is still much scope for further advance of knowledge by both subjective and objective methods.

In skin, ear and eye, it is certain that there is considerable overlap in the area of distribution of individual afferent nerve fibres. This has sometimes been thought to raise theoretical difficulties about the neurological basis of localization of stimuli. It is argued that these difficulties are largely illusory and that overlap may actually facilitate accurate identification of the site of stimulation.

The number of receptors is commonly much greater than the number of nerve fibres ascending to the sensorium. This forms the basis for the areal facilitation or summation which is so obvious a feature of natural stimulation of skin and retina. It also, however, provides opportunity for activity in one pathway to inhibit simultaneous activity in neighbouring pathways. There is evidence that this occurs in cutaneous, visual and auditory relay stations.

More puzzling is the multiplication of pathways for which, after preliminary convergence, there is good anatomical evidence in the senses of smell, vision and hearing. Although the sensorium, as analyser, clearly cannot manufacture detail out of material which is undetailed at lower levels, speculative suggestions are made about the possible avoidance of blurring which may result from such divergence.

Finally, the pathways from the two great paired sense-organs of sight and hearing are compared and contrasted. Physiological evidence indicates that in the former there is almost no confluence of the twin pathways from the two eyes even at the highest levels. In the latter, summation occurs to such an extent that a sound heard with both ears has the same apparent loudness as one of double its physical intensity heard with a single ear.

THE EFFECTIVE USE OF MAN-POWER

IN his presidential address to Section J (Psychology), Mr. Alec Rodger discusses the effective use of man-power. He speaks as an occupational psychologist, as a scientist and technologist concerned with the discovery and use of knowledge about people as workers of every kind and level. The knowledge the occupational psychologist is specially interested in is knowledge about determinants of behaviour conveniently regarded as (*a*) individual and internal, but (*b*) 'left over' when the physiologist has done all the explaining he can manage. It is useful to call these, broadly, capacities and inclinations. The psychologist's task in his occupational specialization is to put our thinking about them on a sound theoretical basis and to bring it to bear, in a skilful fashion, on the problem of 'fitting the man to the job' (through improvements in methods of vocational guidance; personnel selection; and occupational training) and the complementary problem of 'fitting the job to the man' (through improvements in methods and organization of work; equipment design; and working conditions of diverse kinds).

Before reviewing urgent tasks in these fields of interest, Mr. Rodger stresses the importance of recognizing their essential unity. It is unfortunate, he says, that psychologists dealing with one type of problem sometimes display little appreciation of thoroughly relevant work being done by their colleagues. For example, investigators concerned mainly with fitting the job to the man are inclined to assume that all men are equal, or nearly so, in their capacities. They are therefore apt to overlook individual differences found to be crucial by investigators concerned chiefly with fitting the man to the job. This leads to misjudgment of the importance of the nature and size of the population samples used in their investigations, and to unwise expenditure of precious research funds on projects too narrowly conceived.

In the field of vocational guidance, where planning for the effective use of man-power should begin, the most pressing need is for long-term studies designed to reveal, by the application of appropriate criteria, the strong and weak points of currently used concepts and methods. On such studies will depend the solution of many major problems, including the most fundamental of all, that of classifying occupations for vocational guidance purposes. What is particularly required is a series of integrated studies of the differentiating capacities and inclinations of successes and relative failures in a variety of occupations. This notion is equally applicable to the study of certain personnel selection problems, including some arising in the management field. In the field of occupational training, consideration of individual differences in capacity and inclination may well lead to a radical overhaul of existing arrangements in Britain, not least of the apprenticeship system, which is archaic and wasteful.

Mr. Rodger, outlining some of the tasks awaiting the occupational psychologist in the field of methods and organization of work—the field of 'work-study'—suggests that the nature and extent of the arbitrary judgments involved in time-study urgently require investigation. Until we know more about the answers to certain questions that arise here, we shall be entitled to wonder whether a good deal of time-study is not a piece of mumbo-jumbo developed—by agreement between management and workers—to provide an apparently rational basis for the payment of wages the amount of which has already been settled by quite other means. Touching on problems of equipment design, Mr. Rodger thinks it is time we began to take serious account of the fact that automation tends to produce a proportionately higher demand for skilled than for unskilled workers. He concludes by arguing that in the study of all the main factors to be considered in the shaping of wage-differentials—skill, effort, responsibility, dirt and danger, to use Mr. Hugh Gaitskell's list—the occupational psychologist should have a contribution to make; for all these demand the investigation of problems of capacity and inclination, which are the occupational psychologist's meat and drink.

BIOLOGICAL ORGANIZATION

THE biologist is faced with many different but interdependent levels of organization in the systems which are his concern—systems which range from the smallest living particles, like genes and viruses, to the large aggregates denoted as populations and communities, including human society. In his address to Section K (Botany), Prof. K. Mather points out that genetics and cytology have shown us how the nucleus is organized for the perpetuation and recombination of the hereditary materials. They are beginning to show us, too, how the gene is organized as a unit of action depending for its characteristic mediation of a complex biosynthetic process on the arrangement of its parts in a special spatial pattern. The remainder of the cell, conventionally covered by the title of cytoplasm, is an aggregate of materials, processes and even systems, the properties, origins and maintenance of which are extremely varied. The mitochondria may well provide a mechanical basis for at least part of the cytoplasmic organizations; but balanced action here must depend more on chemical equilibration than mechanical control as it does in the nucleus.

The nucleus provides the firm foundation on which cytoplasmic balance can be built; but the cytoplasm can react back on to the nucleus, as indeed we see in fungal heterokaryons, where the genic balance is adjusted by cytoplasmic alterations of nuclear division. Nucleus and cytoplasm are thus complementary and inseparable in cellular organization, one securing the constancy needed for control and the other the flexibility needed for adjusted action. In somatic differentiation this adjustment is related between the cells, some differences being built up and others suppressed to give the characteristic patterns of development. Its success depends on the ability of a constant nucleus to stimulate and manage a changing cytoplasm.

Each cell is at once a complex organization in itself and a specialized part of the higher organization which we term an individual. Individuals in their turn are parts of the organizations which we term populations, within which they are tied together by functional relations in reproduction (as in dicecy, heterostyly, etc.), protection, survival or the maintenance of genic balance. Being functional, these bonds may tie together as collaborators in one function individuals which are complete and therefore competing units in respect of other functions. Individuals of different species may be tied functionally in symbiotic organizations.

All organization, at its many different levels, depends for its origin and survival on the efficient division of labour among the parts (which may themselves be organizations at another level) in the discharge of some function. These parts may be bound together temporarily or permanently by a wide variety of means—morphological, physiological and behaviouristic. Any organization must have the property of continuation or recurrence; this is secured at the genic and nuclear levels by reproduction as wholes, the daughter being already a replica when it parts from its mother, and full continuity being thereby secured from the start. Some parts of the cytoplasm secure their own reproduction in the same way, but others are built up only in certain circumstances, so giving the characteristic versatility and capacity for change. The repeatability of pattern in this change is governed by the continuous organs of the cell, especially the nucleus. Some continuing parts of the cell may, however, have entered it initially by infection, depending for perpetuation on their own heredity, now part of that of the cell.

The cellular organization of nucleus and cytoplasm provides the basis necessary for somatic differentiation, nuclear control having developed in such a way as to replace opportunistic responses by ordered changes of the cytoplasm in large measure divorced from the immediate external environment. The organization of populations and the differentiation of individuals within them similarly depend in the general case on the nucleus, which often here has the added function of switching development into its different individual paths by genic segregation. Populations differ from somata, however, in that they may be propagated characteristically by a gradual replacement of parts, so that new members may be adjusted by the action of old ones, as is, for example, the case where some process of learning is involved. In symbiosis each component must depend on its own genetic balance for continuing adjustment, but this must be governed by relations external rather than internal to the individual nucleus. The symbionts must also be endowed with some means of securing their co-ordinated distribution in propagation. This may be secured by the activity of one partner in carrying the other with it, but other means may well also be possible. The mutual adjustments of symbionts, especially their genetical adjustments, offer an important and virtually untouched field of investigation.

No organization is immutable. Changes are continually occurring in the controlling genes. The alterations are mostly degradatory and are weeded out by selection, which may result either from the direct competition of other units of organization at the same level—for example, of cell with cell—or from the failure in competition of a higher organization because of the deficiency of its parts—for example, the failure of an individual because of maladjustment of its cells. Occasionally, however, change will lead to a more efficient organization which will then be at an advantage in selection. Genetical evidence is unanimous that all the complexity of biological organization and its control has arisen in this Darwinian fashion.

Human society differs from most biological organizations in that continuity and control have come to depend on education (in the broadest sense) rather than heredity, though the underlying genetical requirement is still there. This process of education resembles infection rather than heredity in its properties and offers new opportunities while posing new problems. The controlling laws, customs and so on are subject to change, and have undoubtedly evolved in the competition of individuals, communities and societies in the past. The character of this selection is, however, no longer like that of biological levels, and we are seeking to control it. In doing so, we must not forget that our society is but another level of organization superimposed on all the biological systems below it. To flout the principles common to them all and to shut our eyes to their interactions could lead only to disaster.

THE EDUCATED MAN IN 1955

EACH age and civilization has its appropriate pattern of education, some emphasizing the conservative element, others the forward-looking. In his presidential address to Section L (Education), Mr. J. F. Wolfenden asks if there is, in Britain in 1955, such a pattern discernible, with its attendant type of 'educated man'.

Some hold that the prime distinguishing mark of the educated man is his understanding of human nature, on the ground that the most important fact about this planet is that human beings, each a unique blending of spatio-temporal body with eternal spirit, live on it. The corresponding educational pattern is composed of literature, history, philosophy, economics; it is not remote or 'literary' if it is fertilized by daily contact with practical human affairs.

This picture appears to leave out non-human phenomena, the subject-matter of natural science. The study of natural science begins as a disinterested inquiry. Nobody in 1955 can ignore scientific thinking and its consequences.

Many people brought up in the 'humanist' tradition not only do not know many scientific facts which are important; but also, more fundamentally, are incapable of certain modes of thinking derived from scientific studies and indispensable to an understanding of affairs in 1955. This does not mean that the scientist is automatically a more educated man than the non-scientist; he may be illiterate, incapable of historical imagination or of human insight. But it does mean that there are people who are scientifically illiterate. Nor does it mean that the scientist is the man who should decide politics—local, national or international. That is a misunderstanding of the nature of his training and his skills, especially as he must often, for the purpose of his own studies, ignore human factors.

So far we have been concerned with what will always be the education of a minority. The most important revolution ever to occur in educational theory and practice is the national decision that other forms of education in Britain besides that of the 'grammar-school' type shall be recognized as secondary education. To think in terms of educating everybody is revolutionary. Is there a type of education, based on this view, which will lead to a new kind of 'educated man'?

To be called educated a person must be able (*a*) to appreciate the achievements of the human race; (*b*) to move with some facility in the realm of abstract ideas. These criteria cannot at present be satisfied by the 'secondary modern' type of person or of education. This assertion is not undemocratic (though it is un-egalitarian) because the essence of democracy in education is not to provide the same thing for all but to provide the right thing for each.

It may be that there will arise an un-aristocratic culture, based on a new kind of approach to human affairs; and it may be that a new form of conceptual thinking will emerge, superseding the verbal. But it is hard to believe that this will ever be within the capacity of the majority.

THE IMPACT OF EDUCATION ON FARMING

IN his presidential address to Section M (Agriculture), Mr. W. B. Mercer points out that agricultural education in Britain grew up during the years of depression at the close of the nineteenth century; but little progress was made until the Development Fund Act of 1910 made appropriate monies available. The general scheme then devised divided the field into three sections: formal education, research and advisory work among practising farmers. Apart from such advisory work as the universities and colleges could continue to carry out, the main task of interpreting research and carrying its fruits to the farmers fell on the county councils—and at a later stage on the National Agricultural Advisory Service.

The scheme has evolved broadly on the lines then laid down, but has been profoundly influenced by commercial firms providing agricultural requisites, by public corporations such as that for sugar beet, and marketing boards; even more has it been influenced by State direction and price guarantees, which have permitted pioneer farmers to adventure boldly in the development of new techniques. Technical progress is very closely linked with commercial prosperity. Though we have never squarely faced the problem of optimum agricultural production and man-power, we have always paid at least lip service to the idea of good husbandry and high output, and our policy in Britain to-day aims at steady expansion of production with a probably declining labour force. Thanks to the manner in which advisory forces are organized and to the many influences which can be brought to bear on any selected theme, it is now possible to mould or modify farm practice at a pace hitherto undreamed of.

Educational work started with soil science and plant nutrition; animal nutrition came later; only within recent years have the arts of animal and farm management, the latter with its attendant problems of finance, been subjected to scientific study. The influence of agricultural education on the several enterprises which make up modern farming has therefore varied greatly. Nevertheless, in most sections of the industry the links in the chain of research — interpretation, exploitation, absorption into practice—can be traced fairly clearly.

The study of grass as a crop has proceeded from the early work on hay at Rothamsted and the Cockle Park trials with grazing sheep, the results of which at first appeared contradictory. It was many years before the real difference between a hay field and a pasture with its clovers was appreciated, and several decades before the value of the grass leaf as distinct from the whole plant was demonstrated. Once this was grasped, the way was open to intensive grass production, to a study of interactions between crop and animal and to the evolution of an entirely new outlook on grass and entirely new systems of stock management.

In the widely different field of hygienic milk production, the role of the researcher has been technically much simpler. Here the main problem has been creation of an attitude of mind among producers rather than projection of a stream of new discoveries.

Poultry keeping forms a curious outlier to the general current of evolution. That developed from a hobby and a part insurance against unemployment into an industry mainly by trial-and-error methods exploited by practical men, and only when this stage was reached was scientific aid brought in. Commercial poultry keeping to-day is based on Mendelian breeding.

Many different lines of work converge in problems of land and stock management, and experimental work becomes, therefore, very complex. It is a most

hopeful sign of the times that so many men in farming are prepared to undertake the onerous tasks of management which intensive production involves.

THE FRAGMENTATION OF SCIENCE

IN his presidential address to Section X (Assembly of Corresponding Societies), Mr. Ritchie Calder points out that, by the excess of specialization forced upon them by the rapid advance of research and the vast literature of their subjects, scientists are using only a part of their faculties and allowing the others to languish. They are becoming intellectual cripples. They are so busy synthesizing Nature that they have no time to synthesize their own science.

Present-day science, by its emphasis on experimental research, has forsaken natural philosophy, and in its hurried retreat from scholasticism is forgetting the scholarliness in which it made common-ground with the humanities. Over-specialization gives the scientist the excuse for saying "We have no time for other subjects", and their colleagues in the arts the excuse for saying "How can we understand ?". The fragmentation into more and more branches, each with its jargon, is dividing scientists from each other and from the wider public. This separation from the wider community is fraught with danger for our civilization and for science itself. Science, which exists to remove mystery and magic, is creating its own mysteries and magic. People regard science with a kind of superstitious awe, but count on it for the gadgets. In the absence of proper understanding of the methods and processes of science and of any social integration, the apparent haphazardness of discovery encourages a popular attitude towards science which is distrustful and unhealthy.

The educational system of Britain has become lopsided. Our schools and universities, by too early and continuing segregation, give too little science to one section of citizens and too much to another. This is as much a criticism of the humanities as it is of the science faculties.

The public has an uneasy awareness that the branches of science are out of step. On such an issue of life or death as the hydrogen bomb, it is realized that the physicists have outstripped the biologists—with the sombre risks which Lord Adrian forebode. Prof. A. V. Hill had also stressed the risks of medical science outstripping the means to feed the lives that are saved.

It is suggested that the proposal put forward in the British Association report on Post-War University Education (*Advancement of Science*, 3, No. 9) be re-examined—that there should be honours and pass schools of "Philosophy, Natural and Social" and that this should be reflected in the schools as general instruction on "scientific methods and social implications of science" ; that experts—doctors, engineers, agronomists, etc.—going into the widening fields of mutual aid should temper their expertize by some training in social anthropology, and that we would do well to revive, even at this late date, Comenius' idea of *Pansophicon* (1641), to bring scientists together to assess and explore all natural knowledge and propound it and make it widely known for the adoption by men for their benefit. Then, perhaps, we could get that synthesis of scientific knowledge by which science and humanity could progress together and science become wisdom.

NEWS and VIEWS

(*Continued from p.* 432)

plants, as would be expected because of their inherent metabolic differences and the variations in their physiological states induced by experimental conditions. Sucrose became radioactive in very different amounts in two major groups of plants, namely, those containing only photosynthetic tissue, and those containing non-photosynthetic tissue as well. The amount of radioactive sucrose in the former group was much lower than that in the latter. An unidentified compound became radioactive in appreciable amounts in two of the blue-green algæ, but was radioactive in very small amounts, or not visible at all, on the chromatograms of all other plants.

Changes in Lunar Topography

IN a second paper on changes observed on the surface of the moon, Dr. H. P. Wilkins deals with "Bubbles and Streaks" (*J. Brit. Interplanetary Soc.*, 14, 3 ; May–June 1955). In collaboration with Patrick Moore, Wilkinson has recently investigated a number of rounded hills or 'domes' and has listed about a hundred, most of which have diameters of two to three miles and heights of a few hundred feet. A very remarkable thing about these domes is that they generally have a deep pit at the top, the pits being seated centrally, never at the side ; it is extremely improbable that they could have been caused by meteorites, which would strike the sides also, but there are no signs of this. As very few of these domes were found until recently, the conclusion is that the older observers were not as careful in their work as is generally supposed, or, alternatively, they have been recently formed. If the latter hypothesis is adopted, the most likely explanation is that some volcanic activity still remains on the moon, the slow welling out of the magma forming the dome, and the pit at the top representing the vent. One very remarkable piece of evidence tending to confirm this view is the fact that domes now stand on some of the sites where older observers announced depressions. It is suggested that local swellings of the surface may occur in places and that the resulting domes are cavernous. Dealing next with the streaks, Wilkins points out that these were almost entirely overlooked by the older observers ; and while admitting that they may be due to faulting or the formation of narrow cracks, he suggests that they may be due to some low form of life, such as lichens or fungus. This suggestion was actually made many years ago by W. H. Pickering, who thought patches in the crater Eratosthenes seemed to move about and to choose

their paths in an unpredictable manner. Although Pickering's views were not generally accepted, they are difficult to disprove—even if they are equally difficult to prove; no instrumental equipment at present could definitely disprove the theory of some low form of life on the moon.

High Altitude Research: Symposium in Gulmarg, Kashmir

At the meeting of the Council of the National Institute of Sciences of India, held in New Delhi in October last, it was decided to organize a symposium on "High Altitude Research" during May 27–28, at the Gulmarg Research Observatory, Kashmir (director, Prof. P. S. Gill). Gulmarg Observatory was selected because of its unique position. Situated at a height of 9,000 ft. above sea-level, it compares favourably with several outstanding high-altitude laboratories elsewhere in the world. The site is also suitable as a base station for work at still higher altitudes up to 11,000 ft. at Khillan Marg, one hour distant, and up to 14,000 ft. at Apharvat, three hours distant. The geomagnetic latitude (23° 32′ N.) and longitude (75° E.) are of special significance in cosmic-ray measurement, for the latitude is that at which the most rapid cosmic-ray changes occur with changing latitude, and the longitude is close to that where maximum effects of the Earth's magnetic field are observed. The symposium proper was held on May 28, the morning session being allotted to biological sciences, and the evening to physical sciences; but the boundary between the two sciences was never very sharp. In all, about seventy papers were read, and the proceedings will be published in the form of a bulletin by the National Institute of Sciences of India.

Science Students in New Zealand

The Department of Scientific and Industrial Research of New Zealand has carried out a most valuable survey into the supply, quality and employment of graduates in the various scientific disciplines in New Zealand. Included in the survey were 4,852 students (4,068 men and 784 women) who had begun university courses in science since 1950 (New Zealand: Department of Scientific and Industrial Research. Bulletin 113: The Academic Record of Science Students in the University of New Zealand. By I. D. Dick, R. M. Williams and Dermot Straker. Pp. 64. (Wellington: Government Printer, 1955.) 6*s*. 6*d*.). Among the information collected was the length of study, the subjects studied and the marks obtained, the kind of degrees taken and the time taken to get them. A broad picture of the employment followed by the science graduates showed that about one-ninth went into the Department of Scientific and Industrial Research, one-ninth into school-teaching, one-twelfth went overseas and one-thirteenth went into industry. Those who joined the staff of the University of New Zealand were of high academic quality, as were those who went overseas; school teaching attracted few with first-class honours degrees.

Sodium in Empty Ether Bottles

Mr. D. Rainy Brown, of J. F. Macfarlan and Co., Ltd., 8 Elstree Way, Boreham Wood, Herts, writes stating that bottles which have contained ether are returned to the firm from time to time containing traces of metallic sodium, presumably used to remove traces of water. There is a real risk of fire when such bottles are washed. Although precautions are taken, Messrs. Macfarlan have over a period of years had accidents, although fortunately none of them was serious. They therefore ask those who use sodium for drying ether to have special vessels for the purpose, or to ensure that no sodium is left in returned bottles.

Petroleum Films Bureau

The 1955 edition of the list of 35-mm. and 16-mm. documentary and instructional films issued by the Petroleum Films Bureau contains no fewer than forty-five additions to the previous catalogue. Among the new subjects available are "Ageless Iraq"; "The Search for Oil"; "Assam Oil Survey"; "The Digboi Story"; "The Chemistry of Oil"; "The Modern Oil Refinery"; "Red Ruin" or prevention of rust; "Individual Aircraft Series", including the Comet, Valiant, Brabazon and Hawker Hunter. All 16-mm. films are 'non-flam' stock: 35-mm. films are 'flam' stock. Applications from educational establishments, scientific and technical societies, youth clubs and the like to borrow films should be made to the Petroleum Films Bureau, 29 New Bond Street, London, W.1.

University of Liverpool

The following appointments in the University of Liverpool have been announced: Dr. J. D. Craggs, reader in electronic engineering, to the Robert Rankin chair of electronic engineering; Mr. D. L. Hughes, head of the Veterinary Science Division, Boots Pure Drug Co., Ltd., Nottingham, to the chair of veterinary pathology, in succession to Prof. R. E. Glover, who has become head of the Royal Veterinary College, London; Dr. F. D. S. Butement, a principal scientific officer at the Atomic Energy Research Establishment, Harwell, to the newly established post of senior lecturer in radiochemistry in the Department of Inorganic and Physical Chemistry.

Announcements

The autumn conference of the British Occupational Hygiene Society will be held at the London School of Hygiene and Tropical Medicine on November 4; the subject will be "The Use and Abuse of Protective Equipment". Further information can be obtained from the Honorary Editor of the Society, Dr. C. N. Davies, London School of Hygiene and Tropical Medicine, Keppel Street, London, W.C.1.

It has been the practice for some years past for members of the atomic energy projects of the three countries, the United Kingdom, the United States and Canada, to meet to discuss problems of common interest in the field of health and safety. It is proposed to hold another of these meetings at the Atomic Energy Research Establishment, Harwell, on September 19 and 20, specifically to consider programmes of genetic research. In certain quarters this has been reported as an international conference on radiation-genetics; this is an over-statement of the case. As with previous meetings, this will not be a formal conference with prepared communications, but rather a working party to consider how best to plan future research and to integrate the available effort. It will differ from previous meetings in that a few invitations have been sent to geneticists not actively connected with the three projects. With these as consultants, it is hoped that the most profitable programmes will be devised for future research.

TWELFTH INTERNATIONAL CONGRESS OF APPLIED PSYCHOLOGY

WITTGENSTEIN once argued that no paper read to a scientific society should last more than fifteen minutes, because anyone who had anything to say on any subject could easily say it in that time. He would probably have regarded with some horror the Twelfth International Congress of Applied Psychology, which met in London during July 18–23. Few of the speakers named on the programme took less than half an hour, and there were more than a hundred of them. Yet only four special lecturers had been asked to talk for more than twenty minutes. Perhaps many of the others felt that, having come from the ends of the earth, or thereabouts, they could justify themselves—to themselves, or to the bodies which in certain cases had paid their expenses—only by going on and on. Nor was the unbargained-for time always well spent. In fact, and as usual, the people who spoke the longest had the least to say.

Nevertheless, the Congress was a good one, and on the whole it took satisfactory account of an important point made by the president, Dr. C. B. Frisby (London), in his opening speech, when he stressed the need, in applied psychology, for paying more attention to the attitudes displayed to the investigator by the investigated. There was less discussion of his equally important contention that, in the implementation of the results of research, the psychologist must beware of the hazards of leaving all the work to others. Dr. Frisby said he does not see, in the psychological field, the possibility of an equivalent to the engineer, who can interpret the discoveries of the physicist. There is, in fact, a danger that attempts may be made by others to apply procedures found useful in situations which psychologically are very different. At the same session, the Lord President of the Council, Lord Salisbury, wisely urged that psychologists should be ready to "come down every now and then from the rarified air they normally breathe and be ready to explain, in words the general public can understand, what they are doing and why they are doing it. . . . It is because the ordinary man is bewildered that he is distrustful. He feels there must be a catch in it".

Sir Frederic Bartlett (Cambridge), in a lecture on "Timing", gave an admirable exposition of the outcome of recent experimental studies of skill. Two or more related sequences of events are involved whenever a skilled performance of any kind or level occurs. One provides the performer with information; the other consists of what he does about it. One is a receptor sequence; the other is effector. In 'good' timing, each effector constituent enters the sequence at such a point that it produces the maximum of desired effect with the minimum of associated effort. Minimal effort has two empirical signs: an absence of both forced hurry and forced loitering. Experiment has shown that timing is principally controlled by adjustments of the relatively stationary intervals in effector sequences. During these intervals, central, perceptual and intellectual activity is demanded; and it is from this that timing derives its remarkable range of rapid adaptability. In human social affairs, Sir Frederic argued, timing is just as important as it is in the simpler, bodily skills.

Prof. E. Mira y Lopez (Rio de Janeiro) reported the results of experiments on the effect of foresight and attitude on mental output. His subjects had been asked to predict their own health, work, wealth, social prestige, domestic happiness and individual contentment one week, one month, one year, ten years ahead. His main early findings were that the accuracy of prediction depended more on the nature of the facts predicted, and the predictor's psychological make-up, than on the passage of time; that pessimistic predictions about health were more often confirmed than optimistic ones; that predictions about work output were more often accurate than predictions about social prestige, wealth or domestic happiness; and that in the case of neurotic and psychotic subjects fear seemed to dominate their motivation. All this led to a further inquiry into the association between 'force of belief' and accuracy of prediction. A study of attitudes towards psychological tests produced the conclusion that "extrinsic factors, especially of a conative kind, may influence performance in a battery of aptitude tests almost as much as the factors theoretically involved in its normal execution". There is nothing new here, but the additional evidence will be useful.

At the beginning of a symposium on "The Evaluation of Methods in Applied Psychology and the Problem of Criteria", Mr. J. M. Faverge (Paris) strongly advocated the setting up of good criteria at the design stage of investigations; and in doing so he uttered a warning—never unnecessary, it seems—against the hazards of small samples in psychological research. Prof. J. C. Flanagan (Pittsburgh), who followed, spoke some more very good sense on both these points. He said that for the psychologist in the field there is no more fundamental question than, Are we really helping people to solve their problems? The ultimate criteria must always be of the 'on the job' variety; but intermediate criteria, too, are required. In industry, complications often arise through contamination resulting from informal discussion between people responsible for collecting predictor data and others responsible for collecting criterion data.

Prof. R. Bonnardel (Paris), in another symposium, on "The Organization and Application of Research", made a good and uncommon point in arguing that the industrial psychologist is really the only one of his kind who has much chance of studying normal people in the normal conditions in which they spend most of their waking lives. Growing co-operation between practitioners in the field and psychologists in university departments is improving our ability to make use of the opportunity. However, Prof. Bonnardel shared the view of Prof. L. S. Hearnshaw (Liverpool), expressed elsewhere, that the former are still too little inclined to classify, while the latter are apt to classify prematurely. Dr. H. E. Page (Washington), who followed, spoke from a wealth of experience in the administration of science in the U.S. Office of Naval Research. After describing how this organization works, he talked discreetly of some of its major difficulties. One arises from the very great influence exercised by a very few psychologists, through their membership of controlling committees of one kind and another. He did not claim that this is necessarily a bad thing; but he maintained that it is an exceedingly important one. Another difficulty concerned the implementation of research findings. Dr. Frisby had hinted that this is intensified by the

entry of untrained people into the psychologist's territory. Dr. Page suggested—without mentioning Dr. Frisby's comment, which he may not have heard—that perhaps the psychologist is himself to blame for this. Indeed, he seemed to extend the main point made by Lord Salisbury, for he went on to say: "More scientists should learn to employ the language and concepts of their users. Many have no desire to do this". He added that the personality of the translator may be more important than his knowledge of information theory.

Prof. O. Klineberg (Paris), who took the chair at a symposium on "Attitudes, Opinions and Behaviour", made a useful and all-too-brief contribution of his own when he took two of his main speakers to task for suggesting that it is unnecessary for psychologists, sociologists and others in this field to agree on the use of terms. It is indeed difficult to discern just what Prof. R. Clémens (Liège) had meant when he said that psychology and sociology differ largely in emphasis. Dr. H. Durant (London) had side-stepped the issue by dismissing it as "partly semantic". It will be interesting to discover, some day, how much of the present-day confusion in this area could be got rid of by the simple expedient of clearing our lines of communication in the way Dr. Klineberg proposed.

The Congress, which met in plenary session for major symposia each morning, and split into six parts each afternoon, for four minor symposia and two series of individual papers, was housed in the Beveridge Hall of the Senate House, University of London, in various theatres in Birkbeck College, and in the Masaryk Hall of the School of Slavonic Studies. One of the four special lectures was given by Prof. C. Pellizzi, head of the Human Relations Division of the European Productivity Agency, which had generously provided simultaneous translation facilities in the Beveridge Hall. Another, delivered at the closing session, was by Prof. M. S. Viteles (Philadelphia). Entitled "The New Utopia", this last lecture provided the applied psychologist with yet another warning that he must be careful how and where he walks. He may find it difficult to avoid making value-judgments of considerable importance to himself and others; but he should at least know when he is doing so. "We cannot allow the fact that our young science is making some contribution to the solution of present-day problems to encourage us to think of ourselves as the architects of the New Utopia."

Among the social events was a banquet at the Hurlingham Club, with a speech by Lord Halsbury in five languages.

ALEC RODGER

UNIVERSITY AFFAIRS IN GREAT BRITAIN, 1953–54

THE "Returns from Universities and University Colleges in receipt of Treasury Grant for the Academic Year 1953–54", issued by the University Grants Committee*, shows a further decline of 872 in the number of full-time students—to 80,602, compared with 81,474 in 1952–53 and 85,314 in 1950–51. In the English universities there were 646 fewer full-time men students but 184 more women than in 1952–53; for Wales there were 146 fewer men and 44 fewer women, and for Scotland 201 fewer men and 19 fewer women. Overseas full-time students from within the British Commonwealth increased by 368 to 4,607; but part-time students (1,819) were 23 fewer than in 1952–53. Students from foreign countries increased by 144 to 3,373 full-time, and by 77 to 1,379 part-time, students compared with 1952–53. There was no appreciable change in the percentage distribution of full-time students among the faculties: students of technology increased by 43 to 10,036, but those of pure science decreased by 30 to 17,001, the percentage distribution being 12·4 and 21·1, respectively, compared with 43 per cent for arts, 16·4 for medicine, 3·2 for dentistry, 2·6 for agriculture and 1·3 per cent for veterinary science. Although the percentage of arts students was slightly higher, the number of students (34,673) was smaller by 243. Full-time advanced students of pure science increased by 245 to 3,290, of technology by 224 to 1,381, of arts by 290 to 3,596, of agriculture by 9 to 290, and of veterinary science by 5 to 12, while those of medicine and dentistry decreased by 64 to 899, and only 2,698 students were working for a teacher's diploma compared with 2,900 in 1952–53. Of the full-time students, 63,560 were reading for a first degree, 4,350 for a first diploma, and 12,166 (including those working for a teacher's diploma) were engaged in research or other advanced work, the corresponding figures for 1952–53 being 64,753, 5,064 and 11,657, respectively.

The proportion of assisted students was slightly higher (71·9 per cent compared with 70·4 per cent in 1952–53), though still smaller than in 1951–52 (72·4 per cent); for England as a whole it increased from 72·9 to 74·2 per cent, and in Wales from 81·1 to 88·5 per cent, but in Scotland only from 55·7 to 56·8 per cent. Parliamentary grants, however, were 70·5 per cent of recurrent income and only 12 per cent comes from fees, 4·3 per cent from endowments, 3·6 per cent from local authority grants and 1·6 per cent from donations and subscriptions. For 1951–52 the corresponding figures were 14·8, 5·2, 4·1 and 1·9 per cent, respectively, with 66·5 per cent from Parliamentary grants. The ratio of staff to students continued to improve, the full-time teaching staff numbering 9,514 compared with 9,098 during 1952–53. The increase was chiefly in lecturers (285), but there were 37 additional professors, 69 more senior lecturers and 12 more readers, assistant professors and independent lecturers, with a further decrease of 120 in assistant lecturers and demonstrators to 1,162.

Of the full-time students, 62,173 were in England, 4,431 in Wales and 13,998 in Scotland, the distribution in English universities and university colleges being 14,867 at Oxford and Cambridge, 18,125 at London, and 29,181 at others. Except at Bristol, Leeds, Reading, Southampton and Oxford, where there were slight increases, there were slight decreases in the number of students at English universities and university colleges, but only at Manchester (− 159), Oxford (+ 159) and North Staffordshire University College was the change more than one hundred and only for the last was it significant in relation to the total (495 for 1953–54). In Scotland there were decreases at Aberdeen (− 95), Glasgow (− 156) and Edinburgh (− 56), and increases at Glasgow Royal Technical College (+ 76) and St. Andrews (+ 9). The proportion of full-time students in Britain residing in colleges and halls of residence increased to 28·1 per cent; of these 22,625 students, 8,069 were at Oxford and Cambridge, 11,565 at other

* Universities Grants Committee. Returns from Universities and University Colleges in receipt of Treasury Grant—Academic Year 1953–54. (Cmd. 9477.) Pp. 44. (London: H.M.S.O., 1955.) 2s. 6d. net

English institutions and only 1,189 in Wales and 1,802 in Scotland. The proportion of men in residence was 23·5 per cent and of women 42·0 per cent, compared with 22·9 per cent and 41·3 per cent, respectively, in the previous year, while 32,987 students (40·9 per cent) were in lodgings and 24,990 (31·0 per cent) at home, compared with 40·0 per cent and 32·7 per cent in 1952–53. Of the 21,153 students admitted for the first time during 1953–54 (a decrease of 202 on 1952–53), 19,585 were reading for a first degree and 1,568 for a first diploma; and of those reading for a first degree, 1,382 were less than eighteen years of age and 10,546 were nineteen or more. Of the full-time students, 35,071 out of 47,398 men and 11,036 out of 14,775 women in England were receiving assistance by way of scholarships, exhibitions or other awards from public or private funds; for Wales the corresponding figures are 2,799 out of 3,141 men and 1,124 out of 1,290 women; and for Scotland, 6,024 out of 10,254 men and 1,931 out of 3,744 women. Of the 60,793 full-time men students, 36·3 per cent were in arts, 22·2 per cent in pure science, 17·0 per cent in medicine and 16·3 per cent in technology; for the 19,809 women students the corresponding figures are 63·7, 17·6, 14·7 and 0·7 per cent, respectively. Medicine and dentistry claimed 49·6 per cent of the 4,682 part-time advanced students, arts 35·6, pure science 9·3 and technology 4·8 per cent, respectively, compared with 49·1, 33·8, 7·5 and 9·0 per cent, respectively, during 1952–53.

The recurrent income of the universities and university colleges of Great Britain increased by £1,413,941 to £31,112,024, of which £21,924,115 was from Parliamentary grants, £3,724,585 from fees, £1,332,917 from endowments, £1,130,280 from grants from local authorities and £495,996 from donations and subscriptions. Of the total income from public funds of £23,054,315 and £8,057,709 from other sources, the University of London received £6,957,728 from public, and £2,462,874 from other, sources; the University of Cambridge similarly received £1,436,133 and £1,332,864; for Oxford the corresponding figures are £1,383,461 and £636,418; Manchester, £1,125,886 and £270,863; Durham, £999,429 and £324,969; Leeds, £933,533 and £268,480; Liverpool, £917,590 and £254,203; Birmingham, £900,815 and £240,216; Bristol, £819,537 and £319,179; Sheffield, £579,598 and £170,268; Nottingham, £500,005 and £105,136; and Reading, £546,373 and £65,241. No other English university or university college had an income exceeding half a million pounds, but for the four universities in Scotland the corresponding figures are: Aberdeen, £509,000 and £126,529; Edinburgh, £1,093,169 and £373,686; Glasgow, £1,114,126 and £332,078; and St. Andrews, £545,260 and £116,100. The University of Wales, including the Welsh National School of Medicine, received £1,149,707 from public funds and £302,169 from other sources. Non-recurrent Treasury grants during the year, in respect of capital expenditure, mainly on buildings, sites, equipment and professional fees, amounted to £5,516,356 compared with £5,746,470 in 1952–53.

Of the total expenditure of £30,260,235—an increase of £1,559,801, or 5·4 per cent on 1952–53—administration accounted for 7·3 per cent, departmental maintenance 69·4 per cent, maintenance of premises 12·7 per cent, and miscellaneous expenditure 9·4 per cent. Capital expenditure met from income amounted to £358,289 and allocations to reserve to £342,397; for 1952–53 these figures were £334,516 and £334,015, respectively. Departmental maintenance, which includes salaries of teaching staff, payments for superannuation, the running costs of laboratories, lecture rooms, libraries and museums and the supply of materials, apparatus, books, specimens, etc., increased by £1,244,181 to £21,005,981, the major increase being 13·2 per cent on departmental wages of technicians and laboratory assistants. Salaries and superannuation of teaching staff amounted to £13,616,426 compared with £13,169,879 in 1952–53, but expenditure on repairs and maintenance of buildings only increased by 4·1 per cent to £1,953,017, although that on rates, insurance, heat, light, water, etc., was up by 9·0 per cent to £1,953,017.

Library expenditure, excluding general maintenance of library buildings, etc., and rates, heat, light, repairs, etc., amounted to £1,203,231, or 4·0 per cent of the total expenditure compared with £1,118,428 or 3·9 per cent in 1952–53, 3·8 per cent during 1951–52 and 3·7 per cent during 1950–51. Of this, £600,647 was for salaries and wages, £147,669 for periodicals and £97,229 for bindings. Of the £290,581 expended on books, £75,652 was in the schools and colleges of the University of London, including £11,732 at the Central Library, £11,890 at the London School of Economics, £9,132 at University College, and £5,963 at the School of Oriental and African Studies. The University of Oxford spent £36,088 on books; Cambridge, £18,587; Manchester, £14,256; Liverpool, £9,959; Birmingham, £8,686; Leeds, £9,150; Durham, £10,829; Bristol, £8,441; and Southampton, £7,509. Expenditure on books and periodicals at a number of English universities and university colleges decreased rather than increased during the year, and increases in general are scarcely in proportion to the mounting costs of both books and periodicals. Only the Universities of London (£39,814), Oxford (£13,530) and Cambridge (£12,448) spent more than £10,000 on periodicals, and only Birmingham (£7,110), Leeds (£5,108) and Manchester (£6,250) more than £5,000. The University of Wales spent £12,805 on books and £8,040 on periodicals. In Scotland, Edinburgh spent £13,930 on books and £7,212 on periodicals; Glasgow, £12,943 and £9,559; St. Andrews, £10,650 and £4,336; and Aberdeen, £8,089 and £3,939.

INTERNATIONAL SCIENTIFIC RADIO UNION

PROCEEDINGS OF THE ELEVENTH GENERAL ASSEMBLY

THE eleventh General Assembly of the International Scientific Radio Union was held at The Hague during August 23–September 2, 1954, when some three hundred delegates from twenty-two countries participated under the chairmanship of Father P. Lejay, who was re-elected president of the Union until the next Assembly in 1957.

The main functions of the Union are to promote and organize radio research requiring international co-operation and to encourage the setting-up of common methods of measurement with standardized apparatus. The work is carried out by seven Commissions, and these are concerned with specific aspects of radio research defined by their titles, which were carefully considered and revised at the recent assembly. The present titles, together with the names of the chairmen elected for the session

1954–57 are as follows: I, Radio Measurement and Standards (Dr. B. Decaux); II, Radio and Troposphere (Dr. R. L. Smith-Rose); III, Ionospheric Radio (Dr. D. F. Martyn); IV, Radio Noise of Terrestrial Origin (Mr. J. A. Ratcliffe); V, Radio Astronomy (Dr. M. Laffineur); VI, Radio Waves and Circuits (Dr. S. Silver); VII, Radio Electronics (Prof. G. A. Woonton).

The proceedings of the eleventh General Assembly are being published in eight parts available in either French or English. The first seven parts are to be devoted to the Commission activities; while Part 8, which has just been issued, contains a full account of the administrative proceedings with information on other activities of the Union (10, Part 8 (1955); pp. 125; obtainable from the General Secretary of the Union at 42 Rue des Minimes, Brussels; 14*s*. 6*d*. or 100 Belg. francs).

Although the Union does not now publish the scientific papers which are read and discussed at a General Assembly, a list of titles and authors of the 296 papers and reports submitted at The Hague meeting is given in Part 8; and it is to be noted that the reports of national committees will be published in full in Parts 1–7 according to the Commissions to which they refer. The Union also publishes, six times a year, an *Information Bulletin*, in which are recorded the activities and progress of the Union between successive General Assemblies.

The administrative proceedings (Part 8), referred to above, contains all the resolutions and recommendations adopted by the General Assembly of 1954. Among these is a recommendation accepted from Commission I that the value of 299 792 ± 2 km./sec. should be used in all scientific work for the velocity of electromagnetic waves in a vacuum. It was further resolved that all national laboratories in a position to do so should co-operate in making a comparison of standards of power measurement at frequencies of 3,000 and 10,000 Mc./s.

A full report of the discussions which led up to these recommendations is contained in Part 1 of the proceedings, which was published a few months ago (10, Part 1 (1955); pp. 57; 8*s*. 8*d*. or 60 Belg. francs). This Part 1 contains all the national reports submitted to Commission I, together with the minutes of the meetings of the Commission at The Hague and the resolutions submitted to the General Assembly for adoption. In response to a request received from the International Radio Consultative Committee, it was recommended that members of the International Scientific Radio Union should make observations on the service areas and mutual interference zones of standard frequency transmissions, and also on the type of modulation used by the stations at present in operation. A study of the results of such observations should be of great assistance in the establishment and operation of an effective standard frequency and time service on a world-wide basis.

Among the other activities of the Union reported in Part 1 of the proceedings is an account of the meetings of the committee appointed to consider the programme of work for the forthcoming International Geophysical Year during 1957–58, and brief accounts of the meetings of the Mixed Commission on the Ionosphere and of the Joint Commission on Radio Meteorology held in 1954. The Union committee on the International Geophysical Year has considered various matters relating to the proposed programmes of observations in connexion with meteorology, aurora, ionospheric absorption and 'winds', radio star scintillations, meteors, atmospherics and terrestrial noise. It has also given preliminary consideration to the question of presentation and publication of the results obtained, and made recommendations on all these matters to the international committee on the International Geophysical Year, which will meet again shortly to formulate the recommended world-wide programme.

It is clear from the two parts (1 and 8) of the proceedings already available that they comprise excellent surveys of the relevant fields of radio research, fully documented with references to the original scientific published papers, and the appearance of the remaining parts dealing with the work of Commissions II–VII will be awaited with interest by students and research workers in the field of scientific radio.

R. L. Smith-Rose

PRODUCTION AND MEASUREMENT OF ULTRA-HIGH VACUA

Before 1950, pressures of the order of 10^{-7} mm. of mercury were considered high vacua, and although, undoubtedly, lower pressures were obtained, it was not possible to measure them because the most sensitive vacuum gauges were no longer reliable at such low pressures. To-day, it is possible to produce and measure pressures of 10^{-9}–10^{-11} mm. of mercury, to estimate pressures as low as 10^{-12} mm. and, in the case where interest is concentrated on the chemically active gases present in the space to be evacuated, to estimate partial pressures of these gases of the order of 10^{-14} mm.

A brief but interesting review of these recent developments, together with a summary of the operation of basic pumping apparatus, is given by Dr. J. A. Becker in the January number of the *Bell Laboratories Record* (33, No. 1; 1955). The combination of fore-pump, mercury diffusion pump and cold trap enables pressures of less than 10^{-8} mm. to be reached. With the McLeod gauge, pressures down to about 10^{-6} mm. can be accurately measured, and with the ionization gauge, introduced in 1916, lower pressures by extrapolation can be determined. The ionization gauge fails, however, when the pressure reaches 10^{-8} mm., and work since 1950 has shown that ionizing currents, produced by soft X-rays striking the plate of the gauge, and the emission of photo electrons together with an electron current that reaches the plate even when the plate is at a negative potential with respect to the cathode, which are independent of the pressure, constitute the limiting factors. Bayard and Alpert have proposed an arrangement by which these currents may be minimized. The plate is reduced to a fine wire and surrounded by a cylindrical grid with the filament placed outside the grid. With this modified type of ionization gauge, pressures of 10^{-11} mm. of mercury have been measured.

To attain these ultra-high vacua it becomes important to know the amount of, and sources of, residual gas present in the system to be evacuated and Dr. Becker describes four such sources. They are the release of gas from the cold trap; the diffusion of dissolved gases out of the glass and metal parts of the system; the release of gases adsorbed

on the surfaces of the parts of the system and the diffusion in time of gases from the atmosphere through the glass envelope. Some recent studies of gettering phenomena and observations on the adsorption of gases on clean tungsten, tantalum and molybdenum surfaces conducted in the Bell Laboratories in connexion with vacua of 10^{-12}–10^{-14} mm. are briefly referred to. Evaporated films, however, do not pump rare gases like helium and neon, and to remove them use is made of the ion gauge, which, in addition to its use as a manometer, can also act as a pump. Its speed of pumping is much lower than that of a mercury diffusion pump or of evaporated films, but it has the particular advantages that there is no lower pressure-limit to its pumping action and that it can be incorporated in the tube being evacuated.

COUNTING AND SIZING OF PARTICLES WITH THE FLYING-SPOT MICROSCOPE

By D. CAUSLEY and Prof. J. Z. YOUNG, F.R.S.
Department of Anatomy, University College, London

METHODS for counting and measuring microscopic particles by the use of the flying-spot microscope have been tested with blood, nerve cells, dust particles and prepared standard sprays. The system involves scanning the preparation with twin spots of light, differently polarized, and passing the pulses generated by twin photocells to an anti-coincidence circuit and then to a counting unit. The circuit ensures that a count is registered only on the sweep at which one of the spots crosses the particle; when both cross it the pulses are made to cancel each other[1,2]. Every time that a pulse passes to the counter, the spot on the display tube is made more intense. This provides a visual display that shows which particles have been counted (Fig. 1).

Red cell counts. Red cell counts were carried out with the co-operation of Prof. M. Maizels. Twenty samples of blood were counted by the routine visual method in the hospital laboratory. Two sub-samples were taken from each blood and counted both visually and by the machine in our Department. The visual counts were made with the standard method of recording corpuscles that lie upon the lines of a hæmocytometer. The machine counts were made on a microscopic field of known area, including about six hundred corpuscles. Each machine count was repeated three times on the same field, and the mean taken.

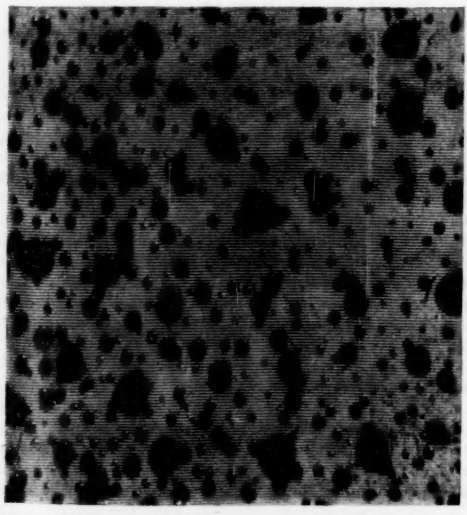


Fig. 1. View of test slide with machine presumed to be counting all particles. The bright spots indicate that a pulse has been passed to the counter

Table 1. Human Red Corpuscle Counts (*M*/c. mm.)
Columns 2, 3 and 4 are visual counts, 5 and 6 machine counts. Counts 3 and 4 were made by a different observer to count 2. The counts in columns 3 and 5 and 4 and 6 were from the same fillings of the slide.

(1) Sample	(2) Visual Hospital	(3) Visual Lab. 1	(4) Visual Lab. 2	(5) Machine 1	(6) Machine 2
1	3·9	3·8	3·6	3·9	3·8
2	5·1	4·4	5·3	4·5	5·3
3	4·1	4·0	3·8	3·7	4·0
4	2·5	2·4	2·6	3·0	3·0
5	1·2	1·1	1·1	1·3	1·3
6	3·2	3·1	3·2	3·0	3·3
7	3·6	3·7	3·4	3·5	3·5
8	3·5	3·8	3·4	3·5	3·6
9	3·9	3·8	3·8	3·7	3·7
10	4·5	4·5	4·5	4·8	4·6
11	4·8	4·7	4·8	4·6	4·6
12	4·7	4·4	4·3	4·4	4·4
13	4·9	4·8	4·7	4·7	4·7
14	4·6	4·7	4·5	4·7	4·7
15	3·3	3·3	3·3	3·4	3·3
16	6·7	7·0	6·8	7·1	6·2
17	4·2	4·1	4·1	4·1	4·1
18	4·5	4·3	4·2	4·2	4·2
19	4·2	4·2	4·2	4·2	4·2
20	5·2	5·2	5·2	4·9	5·0

Table 1 and an analysis of variance show that although the bloods differed greatly, there were no consistent differences resulting from the methods of counting, whether by the hospital laboratory, a second visual observer or the machine.

The replication of the visual and machine counts enabled estimates of variance to be made; there were no significant differences, but the machine variance was possibly the smaller. The standard deviation is about 0·17 *M*/c. mm. by both methods, giving a coefficient of variation of about 4 per cent.

We may conclude that machine blood counts, though much quicker, are as accurate and consistent as visual ones—perhaps more so. The time involved in preparation of the specimen is equal in the two cases, but the counting takes 4 sec. with the machine, at least 4 min. for the visual observer. The repetition of counts on a given field, which is so quickly undertaken by the machine, gives a further protection against error.

Table 2. Counts of the Number of Particles recorded by the Machine and the Total of Nuclei (and Nuclear Fragments) made by Visual Observation

Each count is for a strip 500μ wide extending from the pial surface to the white matter of the rat's cerebral cortex, stained with gallocyanin

Total particles Machine count	Nuclei Visual count
1,336	1,253
1,183	1,220
1,172	1,303
1,164	1,181
1,410	1,424
1,044	1,389
1,197	1,170
1,296	1,357
1,464	1,503
1,389	1,357
1,289	1,242
1,018	1,048
1,315	1,258
1,240	1,104
1,349	1,087
1,372	1,069

Table 3. Steel Dust Particles

(1) Slide No.	(2) Visual 1	(3) Visual 2	(4) Machine 1	(5) Machine 2	(6) Visual 1	(7) Machine 1	(8) Machine 2
	Total number of particles per strip (all sizes) × 10^3				Total of particles 0·5 μ–5 μ per strip × 10^3		
J3/1/1B	497	525	490	510	38	107	119
2*B*	513	475	522	496	30	200	186
3*B*	506	476	639	520	33	174	150
4*A*	928	496	869	792	75	204	243
6*A*	862	682	714	804	99	212	240
7*B*	334	320	354	350	46	110	106
8*A*	1,045	869	765	810	108	217	164
10*A*	599	550	515	487	76	158	128
13*B*	349	385	362	405	47	152	181
19*A*	516	473	494	484	60	170	120
21*A*	818	784	898	860	68	210	248
25*A*	640	675	590	620	77	214	158
33*B*	309	315	374	348	44	104	88
34*B*	639	475	560	496	80	171	192
37*A*	416	386	540	492	44	118	141
38*A*	667	435	395	463	96	196	157
39*B*	316	343	373	393	68	124	139
40*B*	615	555	492	569	107	215	140
42*A*	951	510	365	400	119	132	156
43*B*	461	420	432	392	45	118	182

Counts of nerve cells in sections. The value of the method for counting nerve cells was tested with sections of the cerebral cortex of the rat stained with gallocyanin to show nuclei and Nissl substance. Counts were made with the machine of all the particles in a series of strips between the pial surface and the white matter, followed by a visual count of all the nuclei and parts of nuclei in the same strips. The figures show a general agreement, and the mean difference does not differ significantly from zero.

Dust particle counts. Counting of dust particles may be particularly difficult if there are many near and below the limit of resolution. The maintenance of a constant criterion for counting of imperfectly resolved objects is difficult for both human or machine counting, but is likely to be more variable for the former. Machine counts of such dust were usually considerably higher than those of human observers.

Table 3 shows the results with a series of thermal precipitation samples of steel dust particles. Columns 2 and 3 show visual counts made by observers in Sheffield and London without consultation on standards. There are considerable differences between the counts by visual methods and between these and the machine counts; but the mean differences over columns 2–5 are not significantly different from zero. The replicated machine counts for columns 4 and 5 agree better with each other than do the visual counts of the two observers. Column 6 shows a visual count of the particles of less than 5μ in this material, and 7 and 8 are replicated machine counts of this value. The last two agree well with each other but are higher than the visual counts. Presumably the machine is taking a different criterion of what is to constitute a particle at the limit of resolution.

Table 4. Counting and Sizing of Circular Particles on a Test Slide

Field 600μ × 500μ, 16 mm. objective, 10 × eyepiece. Top, visual counts by three different observers. Centre, counts of different parts of the sample by the machine. Bottom, comparison of means

Visual	1	2	3
Total	415	430	407
Up to 5μ	183	202	182
5–10	145	133	133
10–15	48	45	44
15–20	20	30	27
20–25	10	10	11
25–30	7	8	8
> 30	2	2	2

Machine	1	2	3
Total	408	396	416
Up to 5μ	189	180	192
5–10	136	121	150
10–15	41	50	44
15–20	21	24	15
20–25	12	11	15
25–30	7	8	8
> 30	2	2	2

Mean of 3 counts	Visual	Machine
Total	418	408
0–5μ	189	184
5–10	137	136
10–15	46	45
15–20	26	20
20–25	10	13
25–30	8	8
> 30	2	2

Table 5. Strips of Rat Cortex showing how the Proportion of Particles attributable to Glial and other Nuclei can be estimated by Counting Large Particles separately

All nuclei Visual count	All particles Machine count	Neurons Visual count	Particles more than 7·5μ Machine count
1,197	1,252	727	725
1,296	1,291	760	689
1,457	1,372	790	781
1,056	1,098	840	630

Sizing. Estimates of size are made with this system by comparing the pulses generated with pulses of known duration in such a way that successively smaller and smaller particles are rejected from the count[1,2]. In this way the particles in the test-slide shown in Fig. 1 were measured into 5μ-size groups. Table 4 shows comparison of these measurements with those made visually on a photograph at 300 × by three observers. The agreement is remarkably good.

The same method has been used for estimating the number of neurons, as distinct from all particles, in strips of cerebral cortex of the rat, by counting separately the particles greater than and less than 7·5μ.

Table 5 shows that there is moderately good agreement between the estimates obtained in this way and the counts of neurons made visually on the same strips.

Conclusions. These results show that the method is capable of providing reliable counts and measurements of a variety of particles. Difficulties have been met in balancing the gain factor in the amplification channels from the two photocells when the contrast of the specimen is poor. Other difficulties arise if the particles overlap or touch, and with objects of shapes involving long re-entrants. For some purposes, estimates of the errors due to such factors can be made. The special usefulness of the machine is its speed of operation, making it possible to take more numerous counts than would be practicable by direct vision. For routine counts of easy material, such as blood, it provides substantial economies. For difficult material such as dust, containing particles at the limit of resolution, it may well be that the machine provides approximations that are more consistent than those of a human observer.

Our thanks are due to the Nuffield Foundation for its support, and to Prof. M. Maizels and Mr. F. Fellingham of University College Hospital and Messrs. Maturana, Michie, Sholl, Stanier and Taylor for their help with counting and otherwise, and to Cinema-Television, Ltd., for assistance with equipment.

[May 28

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LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications

Molecular Arrangement in Crystals of the Southern Bean Mosaic Virus Protein

About ten years ago, it was shown[1] that electron microscopic examination of 'psuedo-replicas' could reveal the ordered particle arrangement on the surface of a macromolecular crystal. The necessary preparations were made by metal-shadowing the dried crystals lying on a glass surface, coating with collodion, freeing this composite layer and dissolving the crystalline deposit from the replica thus produced. The molecular order on several kinds of crystal was seen in this way; but a broader application of the method was limited by factors such as the fragility of the collodion film and the need to work with proteins not denatured by vacuum desiccation and the evaporation of metal.

The possibilities inherent in this approach to problems of crystal growth, as well as structure, have been so great that continued efforts have been made to improve this initial technique. Thus 'silicon monoxide' films have been employed[2] to reproduce molecular order in crystals of certain proteins. We have now found that evaporated films of carbon[3] permit a very significant advance in both the quality of replication and the kinds of macromolecular crystals that can be successfully studied.

Molecular order was first demonstrated using crystals of the southern bean mosaic virus protein[1]. In this case, however, as in practically all others, the initial method did not replicate enough different faces to permit a conclusive deduction of the molecular arrangement within the crystal. The numerous crystal faces that can be seen when carbon films are used for preparing specimens of this substance are illustrated in Figs. 1 and 2. These better results are to be attributed in part to the advantageous mechanical and electron optical properties of the carbon membranes, and in part to their chemical inertness and the possibilities this offers of various purifying procedures.

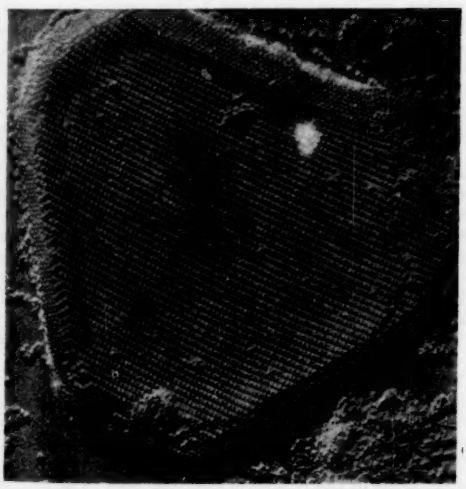

Fig. 1. An electron micrograph of a complete southern bean mosaic virus crystal showing the molecular arrangement on several of its faces. The large face is 110. Magnification × 30,000

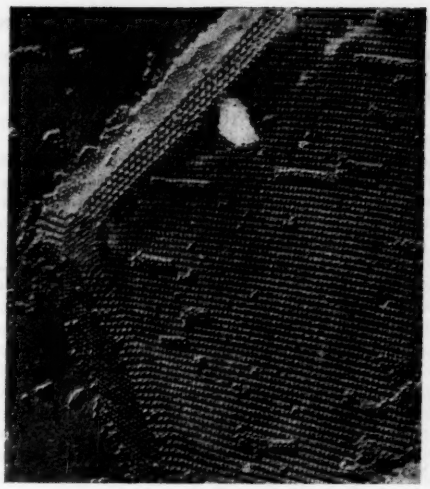

Fig. 2. An electron micrograph of the molecular distribution on several faces developing on one corner of a southern bean mosaic virus crystal. Two [110], [120], [111] and one [100] faces are shown here. Magnification × 30,000

The accompanying electron micrographs are typical of approximately a hundred thus far made of crystals of this virus protein. The crystal symmetry, as determined from measurements of inter-particle spacings and of angles between the edges of crystal faces, proves to be very nearly, if not exactly, cubic. The principal face has invariably been that of a dodecahedron. Faces of the several forms that have been seen have been identified by their relative positions on the crystal and by the particle configurations and spacings they exhibit. This identification has been confirmed by comparisons between the electron micrographs and models built of cork balls which, using the chosen molecular arrangement, reproduce the observed faces. The crystals shown in the figures exhibit faces of the forms [100], [111], [120] and [113], in addition to the dominant [110].

The molecular arrangement in this case is a cubic close packing of essentially spherical molecular particles of diameter about 230 A. The edge-length of the unit cube, containing four of these molecules, is about 325 A.

We have been using this improved method of specimen preparation for studying crystallographic problems connected with the growth of these crystals and for determining the molecular arrangement in a number of other crystalline proteins. These investigations are continuing and will be described in detail elsewhere.

Louis W. Labaw
Ralph W. G. Wyckoff
National Institute of Arthritis and Metabolic Diseases,
National Institutes of Health,
Public Health Service,
Department of Health, Education and Welfare,
Bethesda 14, Maryland.
June 24.

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Terminal Groups of Tobacco Mosaic Virus

The protein of the tobacco mosaic virus is composed of peptide chains. The first step towards a knowledge of the number and kind of these peptides would be the determination of the carboxylic and amino terminal groups.

Harris and Knight[1] found that the only amino-acid liberated by treatment of the native virus with carboxypeptidase was threonine. Assuming a molecular weight of 40×10^6 for the tobacco mosaic virus, the number of terminal threonine residues would be 2,700. We have confirmed this experimental result using our tobacco mosaic virus strain[2]. Since objections can be raised against the enzymatic method, Braunitzer[3] determined the carboxylic-terminal groups by the hydrazine method of Akabori[4] and found 2,500 threonine residues but no other amino-acids. Braunitzer[5] has also recently shown that the last three amino-acids at the carboxylic end of the peptide chains are proline–alanine–threonine. These results have also been obtained by Niu and Fraenkel-Conrat[6], so that the number and kind of the carboxylic-terminal groups remain no longer in doubt.

The question of the amino-terminal groups is still under discussion. In the native virus amino-terminal groups are not detectable. After treatment of the virus with trichloracetic acid (*p*H 1·8, 30 min., 85° C.) proline was the only amino-terminal amino-acid that could be detected by the methods of Sanger[7] and Edman[8]; but no carboxylic-terminal amino-acids besides threonine could be observed by the method of Akabori, which makes it unlikely that an α-peptide bond is split by trichloracetic acid[2,3,6].

Repeating our experiments, Fraenkel-Conrat and Singer[10] found evidence for much terminal proline; but they also found with both methods a "host of other terminal amino-acids". Therefore, they suppose that an unspecific hydrolysis of many labile peptide bonds is effected by trichloracetic acid. The contrast between these findings and our own is due to the difficulty of determining the end-groups of the protein. The method of Sanger is complicated, because dinitrophenyl-proline is unstable and decomposes to dinitrophenol during hydrolysis with 6 *N* hydrochloric acid; but the degradation can be partly suppressed by hydrolysis in a nearly anhydrous medium[11]. In both cases, 90 per cent of the dinitrophenyl products are accounted for by dinitrophenol and dinitrophenyl-proline. Partial hydrolysis yields dinitrophenyl-proline, one dinitrophenyl-prolyl-dipeptide, one dinitrophenyl-prolyl-tripeptide[6], and under certain conditions some yellow compounds, which can easily be taken for dinitrophenyl amino-acids.

The method of Edman gives clearer results when properly applied. Fraenkel-Conrat and Singer[10] contented themselves with the measurement of the total absorption of the produced thiohydantoins and with the identification of the free amino-acids obtained by hydrolysis of the unseparated thiohydantoins. It is known that thiohydantoins of pure amino-acids yield more than one amino-acid on alkaline hydrolysis[12]. After hydrolysis of synthetic proline-thiohydantoin (pro-th) with barium hydroxide we found at least four spots on the paper-chromatogram which were ninhydrin-positive and could be taken for amino-acids. Therefore this method is not suitable for identification. We preferred to separate the thiohydantoins by paper-chromatography first and then to determine the pure compounds after elution.

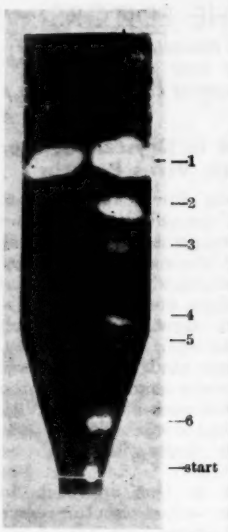

Fig. 1. Chromatogram of the thiohydantoins from tobacco virus protein (right-hand side) and of proline-thiohydantoin (left-hand side)

Fig. 1 is an ultra-violet photoprint of a typical chromatogram: spot 1 has the same R_F-value as pro-th, as can be seen from the reference chromatogram to the left. For the identification we hydrolysed the eluate from the spot with concentrated hydriodic acid (140° C., 90 min.). In this way we obtained nearly pure proline together with a small quantity of a by-product. In a control experiment with synthetic pro-th the same amount of the same by-product was obtained. Spot 1 accounts for 65–70 per cent of the total extinction. Spot 2 is a degradation product of pro-th, produced during the formation of pro-th in acid solution. It accounts for nearly 15 per cent of the total extinction, the other weak spots together to no more than 10–20 per cent. The spot on the starting line has not the typical absorption curve of a thiohydantoin. The R_F-values of the other weak spots correspond to alanine-th, glycine-th and serine-th. These results show that 80–90 per cent of the end-groups in tobacco mosaic virus treated with trichloracetic acid are proline residues, corresponding to 2,400 per mole tobacco mosaic virus.

The impurities (10–20 per cent) need not necessarily be due to an unspecific hydrolysis of tobacco mosaic virus. Control experiments[13] with synthetic peptides showed that glycine and perhaps alanine react with phenyl*iso*thiocyanate, even if they are not terminal. These control experiments with synthetic peptides could not be reproduced by Fraenkel-Conrat and Harris[14]. On repeating our experiments with synthetic peptides, we found an error in the chromatographic technique. Phenylthiourea has the same absorption curve and nearly the same R_F as gly-th, so that the measured amount of gly-th was too high. The present experiments confirm that non-terminal glycine residues react with phenyl*iso*cyanate, but the extent of the reaction is not so high as previously supposed. After five hours reaction (40° C., *p*H 8·5) between phenyl-*iso*thiocyanate and leu-gly-gly the thiohydantoin of the non-terminal glycine constituted 10 per cent of the total thiohydantoins. The corresponding ratio for ala-gly-gly was 30 per cent under the same conditions. The peptide bonds were not hydrolysed by hydroxyl ions under these conditions. When the time of reaction was shorter, the amount of gly-th was correspondingly smaller, so that it is not surprising that Fraenkel-Conrat and Harris were unable to detect it.

The determination of the end-groups by four different methods shows that the protein of tobacco mosaic virus is composed of nearly 2,500 peptide chains with identical terminal groups, as we postulated earlier. It is possible that the sequence of the other amino-acids in the chains is identical too. The tryptic digestion[15] of the protein suggests that every peptide chain contains the same ten digestible peptide bonds.

One of the eleven fragments, containing twenty-one amino-acids, was isolated in the form of a crystalline, apparently homogeneous, dinitrophenyl-derivative.

According to our experiments, the basic unit of the protein has a molecular weight of 15,000–17,000 and contains 130–150 amino-acids. This is in agreement with the analytical data of Knight[16] and with the observations of Schachman and Hersh[17] that the protein of tobacco mosaic virus dissociates in the presence of detergents into subunits of molecular weight 10,000–20,000.

The masking of the amino-terminal group in the native virus is not due to the nucleic acid, as we previously supposed; but we hope to elucidate its nature shortly.

G. Schramm
G. Braunitzer
J. W. Schneider
Max-Planck Institut für Virusforschung,
Tübingen.

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Lunar Occultation of a Radio Star and the Derivation of an Upper Limit for the Density of the Lunar Atmosphere

On April 26, 1955, the moon's position was favourable for the observation from England of the passage of the moon across the large-diameter radio source in the constellation of Gemini. This radio source, No. 2C.537 of the Cambridge survey[1], having R.A. 06h. 13m. 37s. and Dec. 22° 38′ (1950.0), has been identified by Baldwin and Dewhirst[2] as the galactic nebulosity *IC*.443, photographs of which, taken with the 48-in. Schmidt camera at Palomar, reveal a filamentary structure contained within a circular region of 24·5′ radius.

The Besselian elements of this and other occultations have also been computed by Link and Neuzil[3], who have indicated the value of such observations in determining the detailed distribution of radio 'brightness' across the source and in investigating the presence of a lunar atmosphere.

Observations of the source were made at Cambridge at wave-lengths of 7·9 and 3·7 metres, during April 24–28 inclusive. Two independent sets of observations were made at 7·9 metres, using large fixed-aerial systems which provided information for about ten minutes near the meridian transit of the source. At 3·7 metres the two pairs of the large Cambridge radio telescope[4] were used as interferometers of low resolving power. In order to extend the period of observation, phasing cables were introduced into one pair of aerials, so that the reception pattern was deviated 3½° from the meridian. In this way it was possible to observe the source continuously for 25 min.

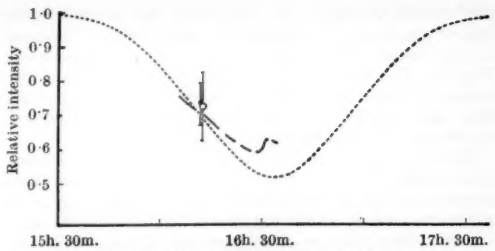

Fig. 1. Relative intensity during the occultation of April 26, 1955, ⊢●⊣ and ⊢○⊣ are measurements at $\lambda = 7{\cdot}9$ m.; — —, measurements at $\lambda = 3{\cdot}7$m.; · · · · · ·, curve predicted from the measurements of Baldwin and Dewhirst

The results obtained are illustrated in Fig. 1. This shows the relative intensity observed at the time of transit, using the two 7·9-metre systems, together with the limits of error. The full line shows the results obtained at 3·7 metres; the gaps are due to intermittent man-made interference which caused a deflexion many times greater than the normal noise-level of the record. In the absence of interference, the accuracy of the results obtained at 3·7 metres is believed to be better than 5 per cent. The dotted line indicates the relative intensity predicted from Baldwin and Dewhirst's distribution derived on the assumption of circular symmetry.

Both sets of observations show good general agreement with the latter curve, but the reduction is less; the most probable explanation of this effect is that more radiation originates in the bright arch on the north-following side (Fig. 2), than in a symmetrical model.

An examination of the 3·7-metre curve shows, in addition, a marked irregularity near the time of maximum phase (16h. 30m.). Consideration has been given to the possibility that this discontinuity might be due to the reception of enhanced solar radiation scattered by the lunar surface; the absence of any evidence for solar activity some hours before and on succeeding days indicates that this explanation is most improbable and suggests that the effect must be explained in terms of irregularities in the 'bright-

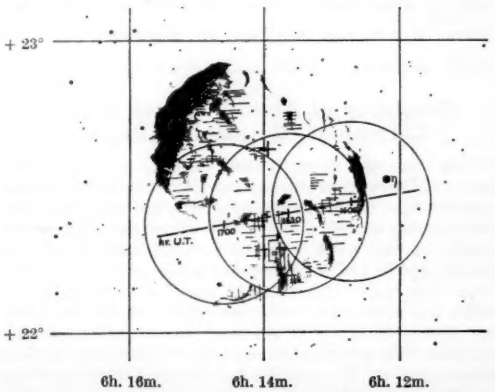

Fig. 2. Moon's position, as seen from Cambridge on April 26, 1955, in relation to *IC*.443. The centre of the radio source is indicated by a cross

ness' across *IC*.443. At this time the moon's west limb is uncovering a part of the well-defined filament on the extreme preceding side, near η Geminorum (Fig. 2). The rise in radio intensity indicates that the radiation from this area is about five times greater than that expected from the circularly symmetrical distribution of Baldwin and Dewhirst.

Both this result and the magnitude of the intensity observed at the time of maximum phase are therefore consistent with the hypothesis that the radio emission originates mainly in the regions of strong $H\alpha$ emission.

If the identification of the increase of radio intensity with the uncovering of the bright area is correct, then the relative times of these occurrences may be used for estimating the density of the lunar atmosphere. Two effects would delay the radio occurrence: diffraction at the moon's limb, which at 3·7-metres wave-length produces a delay of about one minute, and refraction in the lunar atmosphere. Link[5] has computed the latter effect for a number of different models of the lunar atmosphere.

The observations show that the total delay cannot exceed 4 min., and the electron density in the lunar atmosphere cannot then exceed 10^5 cm.$^{-3}$ if the scale-height lies in the range 50–1,500 km. Although the composition may differ considerably from the terrestrial atmosphere, it seems probable that, on the moon's sunlit side, the degree of ionization would be as great as that of the *F*-region. On this supposition the density of the lunar atmosphere must be less than 10^{-12} of the density of the terrestial atmosphere at sea-level, a figure 1/1,000 of the previous upper limit derived by Dollfus[6] from optical measurements.

This work was carried out at the Cavendish Laboratory as part of a programme of research supported by the Department of Scientific and Industrial Research, to which one of us (G. R. W.) is indebted for a maintenance award. We also wish to thank Mr. J. H. Blythe for the use of one of the 7·9-metres aerial systems.

B. Elsmore
G. R. Whitfield

Cavendish Laboratory,
Cambridge.
July 13.

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Reliability of Atomic Masses in the Chromium–Germanium Region

For the past thirty years, beginning with F. W. Aston's observation of small deviations from his whole-number rule, the mass spectrograph has been used for determining atomic masses. One of the first significant results of this work was the experimental proof[1] by Bainbridge in 1933 of Einstein's mass–energy relationship. This led to the frequent use of atomic masses to check the correctness of experiments which measured the energy balance in nuclear transformations. In the intervening years the accuracy with which atomic masses can be determined by mass spectroscopy has greatly improved: however, the techniques for studying nuclear reactions have more than kept pace. As a result, when a discrepancy now exists between mass spectroscopic and transmutation values, the mass spectroscopist often finds himself playing the part of uncomfortable defendant, rather than that of patronizing judge. Such is our position in this communication.

In a recent communication[2] from the University of Glasgow, J. M. Reid and I. F. Wright have reported experiments disproving the existence of the 0·56-MeV. γ-ray which has been rumoured to follow the positron emitted by copper-62. This result, which has also been obtained[3] by R. H. Nussbaum *et al.*, gives rise, as Reid and Wright point out, to a serious discrepancy ($\sim 0{\cdot}65$ MeV.) between the value of the copper-63 and nickel-62 mass difference computed from transmutation data and that calculated on the basis of recent precision mass measurements. They also remind us that a similar discrepancy exists in the case of the zinc-64 and nickel-64 mass difference, and properly advise the cautious use of mass values in this region of the Periodic Table. As mass spectroscopists, we are naturally concerned about these matters, and actually have been aware for some time of the existence in this region of several such inconsistencies, including these two. This communication is intended to direct attention to these inconsistencies and to suggest that a particular element, nickel, may be the principal cause of them.

In the hope of discovering some systematic discrepancy between mass spectroscopic and transmutation data, we have calculated a large number (thirty) of mass differences in the region chromium to germanium, using both sources of information. The mass spectroscopic differences have been obtained, primarily, from the masses obtained[4,5] at the University of Minnesota by Prof. A. O. Nier and his collaborators and, quite secondarily, from some derived[6] in our own laboratory. The calculation of the transmutation differences would normally be a physically exhausting undertaking, since it would involve the use of a great deal of information scattered widely throughout the scientific literature. Fortunately, at the suggestion of the Sub-committee on Nuclear Constants of the National Research Council (U.S.A.), this information has recently been completely tabulated. In our calculations, we have therefore made exclusive use of the table of "Nuclear Disintegration Energies", compiled[7] by D. M. Van Patter and W. Whaling, and the "Table of Total Beta-Disintegration Energies", compiled[8] by R. W. King, both appearing in the October 1954 issue of the *Reviews of Modern Physics*. The numerical results of these calculations are not included here, since they present a rather formidable array; but we can provide them for those particularly interested.

These calculations reveal that, between stable nuclides of the same element (for example, iron-57 – iron-56, nickel-61 – nickel-60, etc.), the transmutation and mass spectroscopic differences are reasonably consistent, particularly if one assumes that the actual mass spectroscopic errors are somewhat larger than the stated statistical ones. The only serious inconsistency exists in the case of the zinc-67 – zinc-66 difference.

In the case of mass differences between stable nuclides of different elements (for example, iron-54 – chromium-53, gallium-69 – zinc-68, etc.), the situation is quite different and far less satisfactory. Here only four of the sixteen calculated differences agree within the stated probable errors, and many are far outside the limits, in particular, those differences involving one or other of the isotopes of nickel. In these five

cases (nickel-58 – iron-57, nickel-60 – iron-57, copper-63 – nickel-62, nickel-64 – copper-63, and copper-65 – nickel-64), without exception, the agreement is poor and always in such a direction as to indicate that the masses of nickel determined mass spectroscopically are too low by $\sim 60 \times 10^{-5}$ atomic mass units. There appears to be no other large systematic effect.

On this basis, we are putting forward the hypothesis that the nickel masses are in error, and hope in the near future to make some experimental attempts to confirm or allay this suspicion by investigating the behaviour of nickel ions in the mass spectrograph under varying conditions of pressure. In the meantime, we feel that, within the framework of the foregoing remarks, the atomic mass values for the other elements in the chromium – germanium group may be used with reasonable confidence.

This work is supported by the Office of Scientific Research, Air Research and Development Command, U.S. Air Force, the National Research Council of Canada and the Ontario Research Foundation.

JOHN T. KERR
JOHN G. V. TAYLOR
HENRY E. DUCKWORTH

Department of Physics,
Hamilton College,
McMaster University,
Hamilton, Ontario.
April 1.

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Radioactive Fall-out in Kingston, Canada

DURING the recent nuclear tests carried out by the United States Atomic Energy Commission, we have checked the radioactive fall-out in Kingston, a small Ontario town situated some 2,200 miles north-east of the testing grounds. Our sampling period extended from February 15 to May 28, 1955, and included all the test explosions reported by the Press.

For a large part of the sampling period the ground was covered with snow; samples were taken from the top layer of the snow, melted and 600-c.c. samples filtered and the filter paper counted with a thin-window β-counter. When the snow left, rain was collected and 600-c.c. samples were filtered and counted. When all precipitation failed, dust was collected on a tray, washed off, filtered and counted.

A record of the activity with the background subtracted is shown in Fig. 1. Arrows along the abscissa indicate the day that a test was conducted,

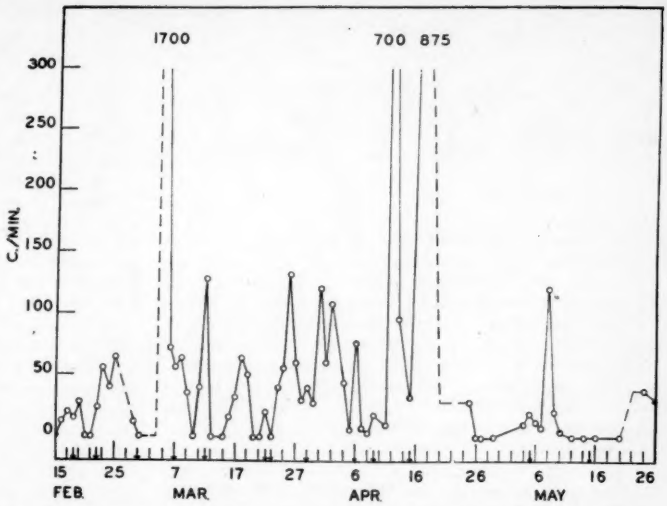

Fig. 1. Plot of counts per min. above background versus time. Dotted lines indicate lack of data because of equipment breakdown. Circles are experimental points and arrows indicate nuclear tests as reported in the Press

and in all cases except April 9 only one bomb was fired. Two explosions were detonated on this date. The largest reported explosions were set off from 500-ft. steel towers and were equivalent to 30 kilotons T.N.T. These were fired on March 7 and 22 and on May 5 and 15. They were detected, but they did not display as high an activity as smaller explosions on March 1 and April 9 and 15.

Meteorological factors seem to outweigh the magnitude of the bombs at such a great distance from the site of the explosion. The high activities detected on March 6 and 7 were found in fresh snow and the activities detected on April 12 and 18 were found in fresh rain. There is generally a one-to-one correspondence between tests and activity peaks, except for an unexplained peak at April 6, which may be due to some previous explosion.

R. L. PRESTON
B. G. HOGG

Royal Military College of Canada,
Kingston, Ontario.
June 6.

Long-Period Fading in Medium-Wave Radio Signals

CONSIDERABLE work has been done on the interpretation of periodic fading observed in continuous-wave radio signals reflected from the ionosphere. Appleton and Beynon[1] observed both slow and fast types of periodic fading in the records of short-wave radio signals which were interpreted by them as due to magneto-ionic origin. Tantry and Khastgir[2] reported long-period fading in the medium-wave records and assumed it to be due to interference of lower trajectory ordinary and extraordinary waves. This interpretation has been confirmed by Satyanarayana and Dharambir Rai[3] by using polarized receiving aerials. The object of the present investigation was to study the periodicity of this slow type of fading in medium-wave records, and compare the observed periodicity with the

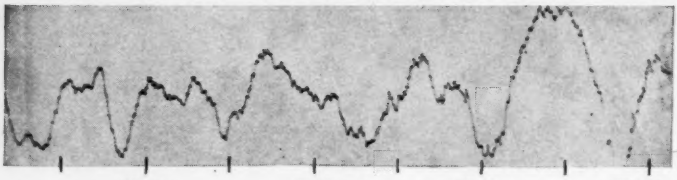

Fig. 1

values obtained from a theoretical calculation of the path-lengths in the ionosphere.

Records of variations of signal strength of continuous-wave transmissions on 1·42 Mc./s. from Madras received at Waltair (600 km. distant from Madras) during the morning hours have been obtained using the conventional type of continuous-wave recording equipment. Hourly records were taken daily during 0700–0800 hr. I.S.T. over a period of one month during May 1954. The records thus obtained showed a long-period fading of the order of 7 min. per cycle on which are superposed other types of short-period fading. A typical record obtained on May 14, 1954, is reproduced in Fig. 1. The long-period magneto-ionic fading on which is superposed a quick fading is clearly seen in the record. It may also be noticed that the fading period gradually decreases with time. On an average, nine peaks are observed in the one-hour records.

The expected periodicity in the records is then determined by using a method of theoretical calculation of the path-lengths in the ionosphere based on the magneto-ionic theory due to Appleton[4] and Hartree[5]. Appleton[6] has applied the magneto-ionic theory for the calculation of path-lengths of both the magneto-ionic components for the case of vertical incidence only; but Booker has worked out the theory for the most general case of propagation at oblique incidence and obtained a quartic equation, which has enabled us to obtain the appropriate curves for the ordinary and extraordinary components of the radio wave for a frequency of 1·42 Mc./s.

The magnetic field at the level of the *E*-region has been assumed to be 5 per cent less than the ground value and the scale-height has been taken to be 15 km. The equivalent paths for the ordinary and extraordinary components in the *E*-region have thus been calculated for 0700–0800 hr. I.S.T. using a method of graphical integration and assuming Chapman's distribution for the electron density in the ionosphere. The critical frequency values at these times for the month of May have been taken from the data reported by the Physical Research Laboratory, Ahmedabad. Table 1 gives the calculated values of total equivalent paths from the transmitter to the receiver along with the path-differences between the two components expressed in terms of the wave-length λ of the radio signal.

From the table it is evident that the path-difference changes by 8·6 wave-lengths during the one hour of recording, giving rise to an average period of 7 min. per cycle, which is in good agreement with the observed values, as can be seen from the record reproduced in Fig. 1. This apparent good agreement is possible because of the assumption of a value of 15 km. for the scale-height following Pfister[8], who assumed this value in order to account satisfactorily for the variations with latitude of the heights of maximum ion concentration of the *E*-region. Calculation of the path-lengths on the basis of parabolic and cosine law distributions of electron density in the *E*-region have given values which are much different from the observed results.

Table 1

	0700 hr.	0800 hr.
Path of the ordinary component	4,104·6	3,852·9
Path of extraordinary component	3,956·9	3,713·8
Path difference between the ordinary and extraordinary components	147·7	139·1

Full details of this investigation will be published elsewhere. We are indebted to the Council of Scientific and Industrial Research for financial assistance of this research work.

M. Srirama Rao
B. Ramachandra Rao
Ionosphere Research Laboratories,
Physics Department,
Andhra University,
Waltair, India.
Feb. 26.

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Flexural Rotational Slip as the Main Deformation Process in the Torsion of Zinc Crystals

Hitherto there has been no successful elucidation of the mechanism by which torsion of single crystals occurs. We have now, however, established that in zinc the main deformation process is rotational slip[1], but in general with pronounced flexure of the slip lamellæ—a process we propose to call 'flexural rotational slip'.

For torsion about [0001], when this is nearly parallel to the axis of the cylindrical crystal (in the case examined, [0001] was 9½° from it) practically pure rotational slip on (0001) is indicated by: (1) the final spiral form of lines initially scribed lightly on the surface, parallel to the torsion axis [0001]; (2) the form of the slip lines on the surface of the twisted crystals; (3) the practically perfect cleavage of the twisted crystals even after 90°/cm. twist; (4) electron diffraction photographs from these cleavages, virtually identical with those from cleaved undistorted crystals; (5) optical reflexes from prism-type etch facets.

The experimental results under (1), (2), (3) and (5) show that the torsion is macroscopically practically uniform; those under (2), (3), (4) and (5) show the rotational slip of (0001) lamellæ; and those under (4) show these lamellæ are virtually undeformed and greater than 200 A. thick, while from those under (5) they are probably much thicker than this. Plastic torsion of 1-cm. lengths cleaved from this 0·97 cm. diameter crystal (bonded by 'Araldite' resin to brass flats mounted in a lathe) first occurred at about

2·1 kgm. cm. couple, which had to be increased to obtain larger torsion, for example, to 2·75 kgm. cm. after 7°/cm. torsion.

A crystal having [0001] at 38° to the rod axis was twisted (held in split collars in chucks) in a lathe to 33°/cm. twist. Cleavage then gave highly smooth surfaces, optically and in electron diffraction closely similar to those from undistorted crystals, except that they were saddle-shaped. While calculation showed that a shape of this form would be expected to be taken up by the inclined (0001) lamellæ if the rod had been isotropic and in homogeneous torsion, the actual shape of the cleavage differed appreciably from that calculated. The difference is clearly due to slip on (0001), associated with the very limited change possible in the dimensions of the lamellæ. This also modifies the flexure imposed on the lamellæ, leading to deviation from circular cross-section of the rod to the observed three-lobed form (the lobes corresponding to three rounded protruberances spiralling around the rod surface).

These features were even more pronounced in a crystal having [0001] at 62° to the rod axis, and twisted through 66°/cm. Curved slip lines (*S*-shaped in side view) were prominent; and though a first cleavage resulted in a fracture nearly normal to the rod, along (0001) planes of the twins developed there during the fracture, a good cleavage was then achieved by impact of a knife parallel to the slip lines, confirming that these were still traces of (0001) planes.

These results for zinc establish for the first time that the main process during torsion is rotational slip, in general with macroscopic flexure of the otherwise undistorted slip lamellæ. Previous observations (for example, in aluminium and iron[2]) now suggest that flexural rotational slip can also occur in cubic crystals even in compression or extension, the slip lamellæ being not necessarily parallel to the same plane as in normal translational slip. We are testing these points further.

A. D. Whapham
H. Wilman

Chemical Engineering Department,
Imperial College,
London, S.W.7.
July 18.

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Complex Formation between Montmorillonite and High Polymers

Previous workers[1] have shown from X-ray measurements that organo-clay complexes may be formed by certain organic compounds entering between the montmorillonite sheets. In the course of investigating the interaction of various high polymers and montmorillonite, a new kind of complex has been observed, involving the edge faces of the clay crystals only. This type of complex, tentatively called a 'peripheral' complex, has the same *c*-axis spacing as pure montmorillonite when dry, but differs markedly in its swelling pattern in dilute salt solutions.

Up to the present, peripheral complexes have been formed only from carboxylated polymers containing no positively charged groups along the chains, such as sodium alginate, carboxymethylcellulose and polyacrylic acid. The complexes were prepared by adding a known amount of polymer to an aliquot of a suspension of sodium saturated Wyoming bentonite and then drying the polymer-clay mixture to form an orientated clay disk 0·04 mm. thick. It was necessary to adjust the *p*H of the suspension to about the half neutralization point of the polymer before complex formation occurred, the latter being indicated by a rapid increase in the viscosity of the suspension. The swelling of flakes 0·5 cm. square cut from the disk was then observed in *N*/2 sodium chloride and distilled water (see Figs. 1 and 2 of flakes in water). A flake prepared from clay alone would, of course, disperse immediately in water.

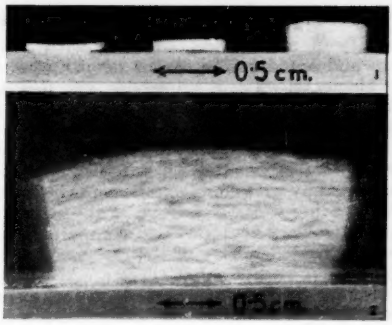

Fig. 1. 15, 7·5 and 4·0 per cent polyvinyl alcohol in water
Fig. 2. 125 per cent maleic acid vinyl acetate copolymer in water

In the case of inter-lamellar complexes, if a large amount of a polymer, such as polyvinyl alcohol, were added, swelling was small both in water and *N*/2 sodium chloride. However, flakes containing a concentration of polymer just sufficient to prevent dispersion, although still swelling little in *N*/2 sodium chloride, did expand appreciably in water. Flakes of peripheral complexes swelled to a limited degree in *N*/2 sodium chloride, approximately equal to the swelling of flakes made from clay alone, whereas in water highly vertically expanded gels were formed. The addition of further polymer did not reduce the vertical swelling but led to a marked lateral expansion in the case of more flexible polymers like polyacrylic acid. A similar swelling pattern has been observed with polymers added to pure illite and kaolinite.

It is suggested that in the peripheral complexes the polymer links the edge faces of the clay crystals by a series of hydrogen bonds to the oxygen and hydroxyl atoms exposed at the edges of the crystals. Further, it is considered that the polymer chains provide only the lateral links holding the clay crystals together, the vertical attractive force necessary to balance the repulsive osmotic pressure being due to electrostatic attraction between the crystals themselves. A full discussion of the experiments and the forces involved will appear elsewhere. The work is of practical importance in that commercial 'soil conditioners' increase the cohesion of soil crumbs by either one of the above methods.

I wish to thank Mr. I. F. Long for taking the photographs.

W. W. Emerson

Rothamsted Experimental Station,
Harpenden, Herts.
April 7.

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Separation of Rare-Earth Elements

The fifteen rare earths, together with actinium, yttrium and scandium, represent 20 per cent of the stable elements. Their properties are less known than those of the other elements mainly owing to the difficulties in their isolation.

The most important step towards the preparation of pure rare earths was the application of ion-exchange chromatography with complexing agents by Tompkins *et al.*[1], Spedding *et al.*[2] and Boyd *et al.*[3] in 1947. The ion-exchange work also provided comparative results on the complex chemistry of the rare-earth elements. One may even consider that these are the first valid results in rare-earth chemistry, as such data as melting points of acetylacetonates previously measured must now be regarded with suspicion because the purity of the rare earths then employed has since been demonstrated to be usually less than 80 per cent of the major constituent.

Elution sequences were hence used for comparisons of rare earths and transuranic elements with such complexing agents as citric acid or hydrochloric acid[4].

Other separation methods were also evolved and depended again on differential complexation of the rare earths, for example, paper electrophoresis[5,6] and paper chromatography[7,8] where separations were recorded with citric, tartaric and lactic acids, hydroxyquinoline and acetylacetone as complexing agents. It is easy to envisage further separation methods based on complex formation: gas-liquid partition chromatography with volatile complexes (for example, acetylacetonates), inverse phase partition chromatography with complexed rare earths, alumina chromatography with complexing developer, etc. These would all be not only of analytical but also of chemical interest as they could provide data for the volatility or extractability, etc., of the complexes employed.

The complexing methods have not, however, contributed to knowledge of the hydrated (or non-complexed) rare-earth ions. Alumina chromatography has provided some data in this direction[9]; but ion-exchange separations with non-complexed rare earths did not prove successful[10].

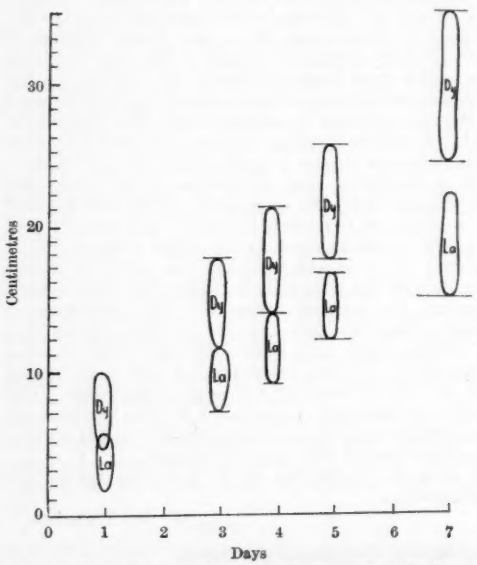

Fig. 1. Separation of lanthanum and dysprosium with ethanol–10 per cent 2 *N* hydrochloric acid as solvent. Descending development and the solvent allowed to run off the paper. The spots shown are larger than those obtained in other experiments as the paper was somewhat overloaded to permit detection with ease even with very diffuse spots

An interesting paper by Götte and Pätze on the paper chromatography of fission products[11] mentions the partial separation of lanthanum and yttrium with ethanol containing 30 per cent of 2 *N* hydrochloric acid as solvent. The authors of the aforementioned paper also state that other rare earths all stay with lanthanum, thus suggesting a specific separation of yttrium from the rare earths.

As in all ion-exchange series, yttrium is usually found between dysprosium and holmium, it was decided to investigate this anomaly of yttrium in the non-complexed state.

Owing to the lanthanide contraction, the difference of the ionic radii between lanthanum and dysprosium will be about as big as the difference between barium – strontium or cæsium – rubidium. Thus when a solvent is used in which (as in the case of Götte and Pätze[11]) the R_F difference between barium and strontium is 0·16, the difference between lanthanum and cerium would not be likely to exceed 0·016. Thus in the search of a solvent for rare earth separation it is best to examine mixtures of lanthanum with a high atomic weight rare earth such as dysprosium or holmium where the difference of the ionic radii is of the same order as that between, for example, barium and strontium. In our previous work with non-complexed rare earths we did not consider this question, and reached a conclusion similar to that of Götte and Pätze, namely, that rare earths do not separate with solvents such as ethanol–nitric acid (M. Lederer, Thèse, 1954).

In the experimental work mentioned below, we employed a lanthanum–dysprosium mixture as the indicator for the exploratory work with various solvents. This mixture has the ionic radius difference discussed above, and when the chromatogram is sprayed with hydroxyquinoline, lanthanum produces a green fluorescent spot (in ultra-violet light), and dysprosium yields a black fluorescent spot. Hence even very small partial separation effects are readily observed.

We first examined the solvent ethanol – 30 per cent 2 *N* hydrochloric acid proposed by Götte and Pätze[11] using descending development and allowing the solvent to flow over the paper for several days. Thus instead of R_F values we shall only state the distances moved, usually with lanthanum as reference. In the above solvent the following mixtures were separated:

Lanthanum – yttrium	lanthanum spot 27·1–30·3 cm.	yttrium spot 32·6–36·0 cm.
Lanthanum – dysprosium	lanthanum spot 24·7–28·8 cm.	dysprosium spot 29·6–35·1 cm.
Lanthanum – ytterbium	lanthanum spot 27·8–30·7 cm.	ytterbium spot 31·2–34·8 cm.

As seen, not only do lanthanum–yttrium separate completely but also lanthanum–dysprosium and lanthanum–ytterbium are well separated. However, the lanthanum spot moves rather rapidly, and for a separation of rare earths closer to each other than lanthanum–dysprosium it was considered desirable to look for a solvent with generally lower R_F values, so as to permit better separation by longer development with the solvent running off the end of the paper.

Ethanol containing 10 per cent of 2 *N* hydrochloric acid appeared to be the best choice of the ethanol-hydrochloric acid mixtures yielding already complete

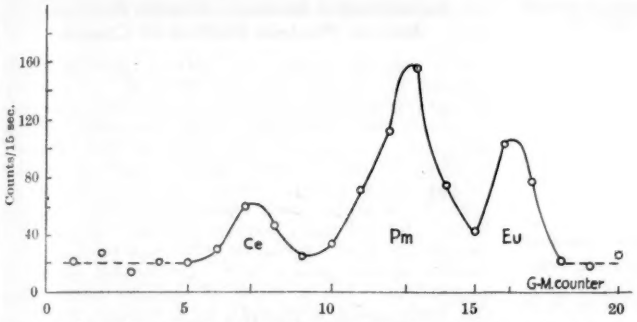

Fig. 2. Separation of cerium, promethium and europium. Solvent: ethanol, 90; 2 *N* hydrochloric acid, 10. Descending for 5 days

lanthanum–yttrium separations with a development of only 29 cm. (solvent front). The R_F values under these conditions are as follows:

Ion	R_F value	Ion	R_F value
Lanthanum	0·11	Dysprosium	0·196
Yttrium	0·16	Ytterbium	0·17

In order to investigate if prolonged development will improve the separations, two sets of experiments were conducted: (1) The development of a lanthanum–dysprosium over various lengths of time and (2) the chromatography of mixtures of closer rare earths.

(1) The separations of lanthanum–dysprosium was carried out for various times between one and seven days, and the fronts and rears of the spots are shown in Table 1.

In the above experiments the amounts of rare earths were relatively large, so as to make detection easier with very diffuse spots as are formed on prolonged development. These results are better visualized by Fig. 1.

(2) To make detection easy, mixtures of two or three rare earths were prepared so as to give adjacent spots of different fluorescence when sprayed with hydroxyquinoline and viewed under ultra-violet light. The mixtures thus studied were lanthanum–europium–yttrium, lanthanum–yttrium and lanthanum–dysprosium with spots of lanthanum, yttrium, ytterbium, lutetium, erbium and europium run on the same sheet of paper.

On four different chromatograms running for three or four days the mixtures lanthanum–europium–yttrium, lanthanum–yttrium and lanthanum–dysprosium were always completely separated into distinct spots. R_F values of lanthanum, europium and yttrium did not change from the pure solution to the mixture. From the few rare earths so far examined it appears that the indicator mixture lanthanum–dysprosium was fortunately chosen, as the R_F values of erbium, ytterbium and lutetium are inferior to those of dysprosium. Thus from the results so far to hand, the movement of the free ions does not follow the sequence of ionic radii, but reaches a maximum near dysprosium–yttrium. The following distances moved in three days on one particular chromatogram may be cited: lanthanum, 7·0 cm.; europium, 8·8 cm.; yttrium, 9·4 cm.; dysprosium, 11·3 cm. (lanthanum mixed with dysprosium, 7·1 cm.); and erbium, 10·2 cm.

Table 1

Time of development	Lanthanum spot		Dysprosium spot	
	Front (cm.)	Rear (cm.)	Front (cm.)	Rear (cm.)
1 day	5·3	1·6	10·0	4·5
3 days	11·3	7·0	17·5	11·4
4 ,,	13·7	9·2	21·0	13·7
5 ,,	16·7	11·9	25·3	17·9
7 ,,	22·0	15·2	35·0	24·4

At this stage of the work a practical problem was suggested to us, namely, the purification of a source of promethium obtained from the fission products at Harwell and known to contain cerium and europium isotopes as impurities. A hydrochloric acid solution of about 1 mc. of the promethium was chromatographed for five days with ethanol containing 10 per cent 2 *N* hydrochloric acid and separated as shown in Fig. 2 into three peaks of activity. A detailed study of the gamma spectra indicating the purity of the fractions obtained will be published later.

The results so far already indicate, however, numerous applications, and appear to be the first successful chromatographic separations in which the rare earths are separated as free ions instead of as complexes.

M. Lederer

Institut du Radium, Laboratoire Curie,
11 Rue Pierre Curie, Paris. April 24.

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Effect of Sunlight on Mixtures of Vegetable Oils and Pigments

Sunlight, according to recent work in these laboratories, is much more active in producing peroxides in the extracted crude lipids of certain vegetables (Lee, F. A., unpublished data), than in ordinary oils[1]. It is known that peroxides are produced when chlorophyll dissolved in oils or in oleic acid is exposed to light[2,3] and that, in the presence of chlorophyll, certain oils autoxidize[4]. Since the crude lipid preparations from green vegetables contain carotenes as well as chlorophyll, the effect of sunlight on colourless or nearly colourless oils mixed either with carotene or with carotene and chlorophyll was investigated. Chevallier *et al.*[5] studied the changes taking place in mixtures of triolein and carotene. They found that β-carotene retards the oxidation of triolein in the dark, but accelerates it in the light of a mercury-vapour lamp. In addition, chlorophyll dissolved in triolein prevents the oxidation-accelerating action of β-carotene in the presence of mercury-vapour light. In that work the chloro-

Table 1. Effect of Pigments on Peroxide Formation in Almond and Brazil Nut Oils exposed to Direct Sunlight

	Unexposed blank	Untreated exposed	Chlorophyll added ‡ mgm.	Carotene added†	Chlorophyll ‡ mgm. carotene†
Almond oil extracted	5	56	565	57	65
expressed	17	63	577	106	252
Brazil nut oil extracted	Trace	107	559	95	85
expressed	8	147*	639	133	133

* Average of two rather poor checks. The accuracy of this figure may be open to question.
† Carotene added as follows: 3 mgm. to ether-extracted oils, 1 mgm. to expressed oils.
Note: The figures for the exposed samples in this table are net values, in that the figures for the unexposed control and the figures for the pigments exposed alone were subtracted.

phyll and the β-carotene solutions were separated by glass. The chlorophyll solution completely absorbed the violet and blue rays emitted by the lamp.

Almond oil was prepared by shelling fresh sweet almonds, removing the integuments by carefully scraping, cutting into small pieces and drying by lyophilization. Following this, they were ground and extracted with redistilled, peroxide-free, anhydrous ethyl ether for twenty-four hours in the dark. Brazil nut oil was prepared in a similar way.

20-mgm. samples of each of these oils were transferred to white porcelain evaporating dishes 100 mm. in diameter. One set was analysed for peroxide immediately, using the method described in previous publications[6,7]. Another set to which nothing was added was exposed to direct sunlight (10,000 ft.-candles) for an hour along with the other samples. Pigments were added to other lots, as shown in Table 1.

Another set was run in which the conditions were the same with the following exceptions: the nuts were not lyophilized, and the oils were expressed with a Carver press instead of being extracted with ethyl ether, and 1 mgm. of carotene was used instead of 3 mgm. The data are presented also in Table 1.

On exposure to 10,000 ft.-candles of sunlight for one hour, chlorophyll causes approximately a ten-fold increase in the peroxide value of almond oil and five-fold increase in the peroxide value of Brazil nut oil. Probably because of its reducing action, carotene retards the production of peroxide. The peroxide values of carotene-oil mixtures were about equal to those of the untreated oils. With the addition of mixtures of carotene and chlorophyll, a higher peroxide value was obtained for almond oil and either a lower or an equal value for Brazil nut oil than with the addition of carotene alone.

Frank A. Lee
Food Technology Department,
New Jersey Agricultural Experiment Station,
Rutgers University,
New Brunswick, New Jersey,
and
New York State Agricultural Experiment Station,
Cornell University,
Geneva, New York. April 1.

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Relationship between Orotic Acid and Animal Protein Factors of Casein

For several years the various manifestations induced in rats by the deficiency of new dietary factors present in crude casein, that is, animal protein factors of the casein, have been investigated in this laboratory. A natural diet, such as that proposed by Randoin and Causeret, namely: ground cereals (wheat, maize, barley, oats, rye), 88 per cent; wheat germ, 5 per cent; crude casein, 5 per cent; calcium lactate, 1·5 per cent; sodium chloride, 0·5 per cent; carrots and fresh vegetables *ad libitum* twice a week, enables normal growth and reproduction for many generations; but a series of changes occur when the crude casein is replaced by a casein purified by repeated washings with water[1-4]. These changes may be summarized as follows: (1) alteration of the body-weight curve; (2) high mortality in the newly born; (3) histological lesions of the cells, mainly of the hepatic cells where lysis and necrobiosis occur; (4) cutaneous lesions and high incidence of neoplasms in the surviving rats of the first generation.

These changes are accentuated in a second generation and might be referred to as a 'late deficiency'. They seem to be elicited by the gradual exhaustion of the reserve of the so-called animal protein factors present in the organism fed on a normal diet and are transmissible from the mother rats to their offspring.

Investigations conducted in our laboratory have demonstrated that these factors are not identifiable either with the vitamins essential for the rat, including B_{12}[1,2], or with methionine and cystine[5] or with the various mineral elements present in the Osborne–Mendel salt mixture[3].

Recently, our attention has been directed to the significance of orotic acid in this type of deficiency. This has been occasioned by the fact that: (*a*) small quantities of whole cow's milk, which, as already known, is one of the richest natural sources of orotic acid[4], are able to solve one of the most dramatic

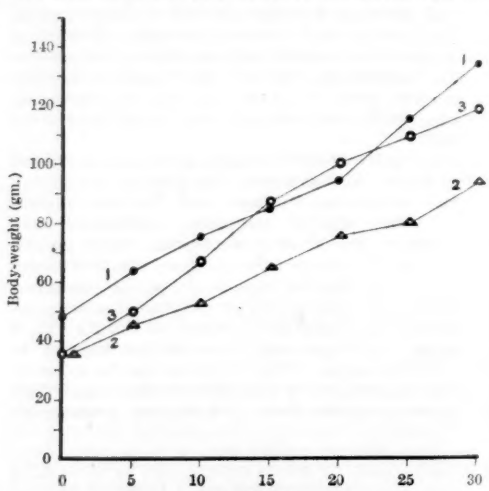

Fig. 1. Body-weight curves for rats fed on: (1) —·—·—, Randoin and Causeret diet containing crude casein; (2) —Δ—Δ—Δ—, Randoin and Causeret diet containing purified casein devoid of the animal protein factors; (3) —O—O—O—, Randoin and Causeret diet containing purified casein devoid of the animal protein factors and supplemented with orotic acid.
Each curve represents the mean body-weight values for twenty animals, male and female in equal number

aspects of the deficiency, namely, the high mortality of the newly born[1]; (*b*) in our purified casein, which seems to be solely responsible for the lesions observed, the orotic acid content is much decreased[7].

We have therefore now investigated the influence of orotic acid upon the body-weight curve of rats lacking the animal protein factor.

Rats of the Sprague–Dawley strain, devoid of the animal protein factor, being the sole survivors of the first generation, were fed from weaning (21 days of age) on our deficient diet, which was supplemented with 1,000γ of orotic acid (manufactured by Biochemical Inc., Co., Cleveland, Ohio) per 100 gm. of food. The results obtained are shown in Fig. 1.

The body-weight values for rats fed on a normal Randoin and Causeret diet containing crude casein, and supplemented with 1,000γ orotic acid per 100 gm. of diet, are not included, since orotic acid failed to show any influence in these conditions.

From the above results, it appears evident that orotic acid may be one of the growth-factors for rats deficient in animal protein factors, but not for normal rats. This suggests a possible relationship between orotic acid and the animal protein factors of casein.

Further work is in progress in our laboratory in order to ascertain the role of orotic acid in the numerous lesions induced in rats by the deficiency of casein animal protein factors.

R. Viviani
M. Marchetti
A. Rabbi
G. Moruzzi

Istituto di Chimica Biologica,
Università di Bologna.
March 18.

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A New Method for the Microbiological Assay of Thiamin (Vitamin B_1)

The purpose of this communication is to present a few experimental results obtained during the course of some investigations carried out to determine the nutritional requirements of a fungus, *Tuberculina persicina* (Ditm.) Sacc., isolated in 1949 by Dr. B. d'Oliveira from æcidia of *Puccinia rubigo-vera tritici*, and since maintained in the fungus collection of the Plant Pathology Department of Estação Agronómica Nacional, Sacavém, Portugal. These results seemed to be of special significance, and may eventually lead to the development of a new method of bio-assay of thiamin (vitamin B_1).

Several vitamins were tested as growth-promoting factors for *T. persicina*, namely, thiamin, biotin, riboflavin, pyridoxin, nicotinic acid, inositol and pantothenic acid (this latter in the form of its calcium salt). The vitamins were added aseptically to a synthetic basal medium (autoclaved for 20 min. at 107° C.) of the following composition: KH_2PO_4, 1·5 gm.; $MgSO_4.7H_2O$, 0·5 gm.; KNO_3, 2·0 gm.; dextrose ('Difco'), 15·0 gm.; agar ('Difco'), 15·0 gm.; distilled water, 1,000·0 ml. The final *p*H was adjusted to 5·2. Purified agar (according to Robbins and Ma's technique[1]) was consistently used throughout the experiments to avoid possible sources of error. After incubation for twenty days at 20° C., it was noticed that the plates to which thiamin had been added not only showed good growth of the fungus colony but also displayed an unusual pigmentation of the medium, whereas those lacking this factor remained colourless, even when all the others were present.

In further experiments, devised to ascertain the optimum levels of thiamin concentration for pigment production, liquid cultures were used preferentially; series of flasks were thus prepared by adding various amounts of vitamin B_1, within the range 5×10^{-4} γ/l. to 5×10^{4} γ/l. to the basal medium without agar. The cultures were incubated for thirty days, at the end of which time very striking differences in the coloration of the liquids were observed. When measured spectrophotometrically, these differences seemed to be directly related to the amount of vitamin B_1 present in the original culture medium. There was almost no pigment formation in the liquids of the control flasks, nor in those in which the thiamin concentration was lower than 5×10^{-3} γ/l. Above 5×10^{3} γ/l. of thiamin, there was a marked decrease in pigment formation, as may be seen from Table 1.

Table 1

Thiamin concentration (γ/l.)	Optical density at λ = 3700 A.	Thiamin concentration (γ/l.)	Optical density at λ = 3700 A.
0	0·083	5	0·900
5×10^{-4}	0·083	5×10	1·300
5×10^{-3}	0·082	5×10^{2}	0·280*
5×10^{-2}	0·208	5×10^{3}	0·250*
5×10^{-1}	0·430	5×10^{4}	0·178*

* Culture medium diluted to 1 : 10 to permit a good reading.

Further experiments are being carried out to determine the influence of the different sources of carbon and nitrogen on pigment production by *T. persicina* in the presence of thiamin. Results will be discussed in a future publication.

J. R. Villanueva
Instituto de Edafalogia y Fisiologia Vegetal,
Consejo Superior de Investigaciones Cientificas,
Madrid, Spain,
and
Estação Agronómica Nacional,
Sacavém, Portugal.
March 29.

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The Duplication Mechanism of Deoxyribonucleic Acid

Recent investigations have shown it to be more and more probable that genes are deoxyribonucleic acid molecules. Furthermore, Watson and Crick[1] have given strong arguments for assuming that these molecules consist of two polynucleotide chains wound helically in a large number of turns around a common axis and tied together by hydrogen bonds between pairs of corresponding purines and pyrimidines, so that adenine can pair only with thymine and guanine only with cytosine. In order to explain the duplication process of the deoxyribonucleic acid molecule, Watson and Crick have suggested that the two polynucleotide chains may be separated and each chain catalyses

the synthesis of a complementary chain. The difficulty in this process is the separation of the two parallel helices which are highly intertwined and locked together by a large number of hydrogen bonds, the breakage of all of which would need a large amount of energy. To overcome this difficulty, Delbrück[2] has suggested a somewhat different mechanism, whereby the two chains are separated and later re-combined by a series of breaks and reunions in a zipper-like fashion. However, this mechanism also seems both too complicated and somewhat improbable since, for example, all these breakings of bonds would again need a considerable amount of energy.

A simpler mechanism for the duplication of deoxyribonucleic acid molecules is made possible by the recent suggestion of Gamow[3], that there seems to exist a one-to-one correspondence between the double polynucleotide chain of a deoxyribonucleic acid molecule of the Watson and Crick type and the single polypeptide chain of a protein (or enzyme) molecule of the Pauling and Corey type, whereby the linear information contained in a deoxyribonucleic acid molecule is translated one-to-one into that of a protein molecule, each one of the possible (about twenty) quadruples of purines and pyrimidines corresponding to one of the possible (about twenty) amino-acids. By this one-to-one correspondence, Gamow shows that a protein (or enzyme) molecule can be built up 'on the inside' of the double helix of a deoxyribonucleic acid molecule so that the order of the amino-acids is uniquely determined by the order of the purine–pyrimidines. However, since this correspondence is one-to-one, this process may obviously be inverted, the protein molecule now serving as the template or mould for the building up of a deoxyribonucleic acid molecule 'around' the protein molecule as axis, so that the order of the purine–pyrimidines is now uniquely determined by the order of the amino-acids. The new deoxyribonucleic acid molecule would therefore be an exact replica of the original one, apart from possible 'casting errors' which would lead to a 'mutated' form of the original gene deoxyribonucleic acid molecule.

In the mechanism here suggested, the protein 'pictures' of the gene deoxyribonucleic acid molecules built up by the Gamow process would thus serve a double purpose: (*a*) as suggested by Gamow, they may be the enzymes which catalyse, and thus control, the biochemical reactions in growth and development of the cytoplasm of the cell; (*b*) they may be the templates or moulds for the duplication process of the gene deoxyribonucleic acid molecules. Furthermore, the duplication of virus deoxyribonucleic acid molecules can proceed by this same mechanism, thus explaining the fact that viruses can only duplicate themselves when their deoxyribonucleic acid gets inside the cells of other organisms, because only here can the virus deoxyribonucleic acid molecules find both the protein material for the templates and the nucleic acid material for the casting of replicas.

Niels Arley

Department of Physics,
University of Copenhagen.
April 1.

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Anti-tumorigenic Action of Iodinated Compounds

As one of us (F. B. O.) had observed that the administration of a mixture of aqueous solutions of potassium iodide and potassium iodate induced favourable effects in cancer patients, we decided to carry out an experimental study in rats.

White rats received an implant of spindle-cell sarcoma of a strain obtained by courtesy of Prof. E. Cruz Coke, of the Institute of Physiological Chemistry of the University of Chile. Animals were given a dose of 0·2 ml. per 100 gm. body-weight of a solution containing 0·482 *M* potassium iodide and 0·083 *M* potassium iodate, three times a day, via œsophageal tube. In this way, 43 mgm. of iodine per 100 gm. of body-weight was administered to each of these animals every 24 hr. Previous experiments with nineteen animals thus treated, giving the iodine solution either two weeks before or on the same day as the implantation of spindle-cell sarcoma, showed that the treatment produces a marked inhibition on the growth of the tumour; in more than 70 per cent the tumours disappeared leaving no traces, whereas twenty-one untreated animals developed large tumours and died. Of the first nine animals treated, six still survive—116 days after treatment was suspended.

We then used another experimental series consisting of two groups of thirty-eight animals each. In one group, the treatment was begun forty-five days before the implantation of the sarcoma. The tumours

Table 1

	Animals		Weeks							
			1st	2nd	3rd	4th	5th	6th	7th	8th
First series	Control	No. of survivors	11	11	8	5	3	1	1	0
		Size of tumours*	1·9	3·9	5·7	6·0	7·4	6·5	6·8	—
	Treated	No. of survivors	10	10	10	8	8	8	8	8
		Size of tumours*	1·8	2·0	1·9	0·6	0·2	0·1	—	—
Second series	Control	No. of survivors	10	10	10	5	5	4	1	0
		Size of tumours*	—	1·4	2·6	3·3	6·8	7·5	8·2	9·5
	Treated	No. of survivors	9	9	9	9	9	9	6†	6
		Size of tumours*	—	1·1	1·9	2·8	2·4	2·3	3·0	—
Third series	Control	No. of survivors	35	35	30	26	22	14	9	—
		Size of tumours*	1·5	2·1	2·7	3·3	4·2	5·0	4·9	—
	Treated‡	No. of survivors	34	34	34	34	34	34	34	—
		Size of tumours*	1·5	1·5	1·2	1·3	1·5	1·8	1·6	—

* Average of greatest diameter of the tumour in cm.
† Three animals died accidentally.
‡ In thirty-two animals of this group there were no traces of the tumour by the seventh week of observation.

grew in thirty-four animals of each group. During an observation period of sixty-four days, thirty-four of the non-treated rats died on account of large tumours, ranging from 15 to 45 gm. in weight. All the treated animals are still alive. Besides the four treated rats in which there was no initial tumour growth, all the treated animals of this group show no traces of the tumour at present, due to its regression.

The iodide–iodate solution at the specified dose and rate of administration has been well tolerated, to the extent that it has produced no hindrance of bodily development.

Studies of the metabolism of these and other halogenated substances, their influence on the weight of different organs and their action on various tumours, as well as the effect of smaller doses of the iodinated solution, are being carried out.

F. B. Oberhauser
H. R. Croxatto
M. Q. Gaillard
V. M. Silva

Laboratory of Chemistry and
Laboratory of Physiology,
Faculty of Philosophy and Education,
University of Chile.

Oxidative and Photosynthetic Phosphorylation by Chloroplast Fragments

Recently, Arnon, Allen and Whatley[1,2] described photosynthetic phosphorylation by isolated whole chloroplasts; but chloroplast fragments were only feebly active in their experiments. On the other hand, I have prepared a chloroplast fragment fraction which caused oxidation of Krebs's cycle acids associated with phosphorylation[3]; but this preparation was inactive in photosynthetic phosphorylation. In modifying the preparative procedure, I have now obtained a chloroplast fragment fraction which caused disappearance of inorganic phosphate either in the presence of citrate or in the light.

Spinach leaves were ground, and the particles were centrifuged and washed as described previously[3], except that 0·02 *M* potassium ascorbate was used in these procedures instead of citrate (cf. the legend of Fig. 1). The fraction precipitated between 250 *g* and 1,000 *g* was referred to as the whole chloroplast fraction, and chloroplast fragments were sedimented at 1,000–10,000 *g*. As shown in Fig. 1, the whole chloroplast fraction decreased inorganic phosphate upon illumination, while, in the dark, the content of inorganic phosphate remained nearly constant even in the presence of citrate. On the contrary, the chloroplast fragment fraction caused disappearance of inorganic phosphate either in the presence of citrate or upon illumination.

The conditions necessary for the photosynthetic phosphorylation by the chloroplast fragments seemed somewhat different from those for the oxidative phosphorylation. The photosynthetic phosphorylation was accelerated markedly by the addition of riboflavin monophosphate but not by coenzyme I or II, and the system seemed to be more stable at higher *p*H values, at lower temperatures, in the absence of added fluoride and in the presence of ascorbate. Under these conditions the photosynthetic phosphorylation proceeded approximately linearly with time during the reaction period of sixty minutes (Fig. 2). Calculated from the data of Fig. 2, the

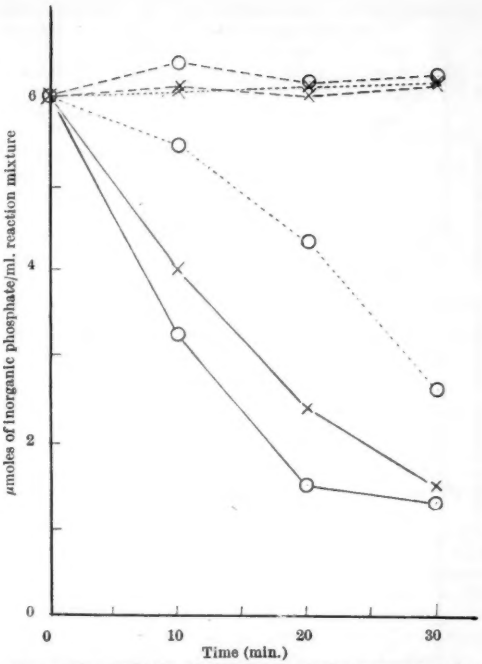

Fig. 1. Phosphorylation in the presence of citrate or in the light. ○,○ Chloroplast fragments containing 0·34 mgm. chlorophyll per ml. of the reaction mixture; ×,×, whole chloroplasts (0·18 mgm. chlorophyll/ml.); solid lines, in the light (*c.* 2,000 lux) without citrate; broken lines, in the dark without citrate; dotted lines, in the dark with 0·02 *M* citrate.
Spinach leaves were ground in a solution containing the following components in final concentrations: sucrose, 200 m*M*; sorbitol-borate buffer of *p*H 7·2, 50 m*M*; ethylene-diamine tetra-acetic acid, 20 m*M*; and potassium ascorbate, 20 m*M*. The particles were washed with, and suspended in, a solution of similar composition, except that the concentration of ethylene-diamine tetra-acetic acid was 10 m*M*. The reaction mixtures contained the following components in final concentrations: $MgCl_2$, 4 m*M*; KH_2PO_4, 6 m*M*; adenosine triphosphate, 1 m*M*; ethylene-diamine tetra-acetic acid, 3·3 m*M*; sucrose, 200 m*M*; glucose, 30 m*M*; sorbitol-borate buffer of *p*H 7·2, 67 m*M*; potassium ascorbate, 15 m*M*; NaF, 8 m*M*; yeast hexokinase, *c.* 14 units/ml. and crude coenzyme concentrate from pig liver, 0·5 mgm./ml. Erlenmeyer flasks of 50 ml. capacity each containing 3 ml. of the reaction mixture were shaken in the air in a bath of 15° C., and, at intervals, aliquots were withdrawn, deproteinized and inorganic phosphate was determined colorimetrically

chloroplast fragment fraction, upon illumination, caused disappearance of 13·7 micromoles of inorganic phosphate per hour per 0·5 mgm. of chlorophyll at 10° C., *p*H 7·6, under aerobic conditions. The value for the whole chloroplast fraction was 20·9 micromoles. These values are in fair agreement with that of Arnon *et al.* (16·7 micromoles) for whole chloroplasts under anaerobic conditions.

Thus, at present, it seems to me that the photosynthetic phosphorylation is the coupling of the Hill reaction with the oxidative phosphorylation. My chloroplast preparations showed such high activities in photosynthetic phosphorylation, as described above even under aerobic conditions, and, in contrast to the finding of Arnon *et al.*, I have, thus far, not obtained results indicating that anaerobiosis was more favourable for obtaining higher activities of the reaction. However, in view of the fact that chloroplast fragments were always more active in photosynthetic phosphorylation than in oxidative phosphorylation, the photo-reduction product of the Hill reaction, that is, the substrate for the subsequent oxidative phosphorylation, may be different from,

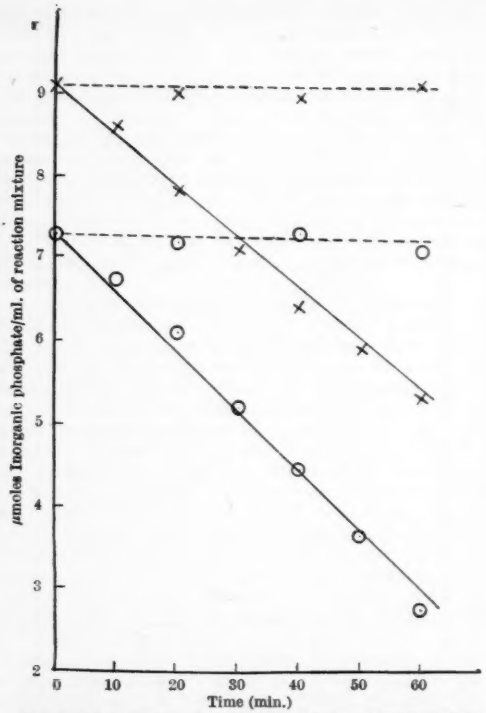

Fig. 2. Time course of the photosynthetic phosphorylation. ○,○, Chloroplast fragments (0·165 mgm. chlorophyll/mL.); ×,×, whole chloroplasts (0·090 mgm. chlorophyll/mL.); solid lines, in the light (*c.* 2,000 lux); broken lines, in the dark. Grinding and washing were carried out in similar solutions, as described in Fig. 1, except that 0·05 *M tris*-(hydroxymethyl)-aminomethane (tris) buffer of *p*H 7·6 was used instead of sorbitol-borate buffer of *p*H 7·2. Reaction mixtures contained the following components in final concentrations: $MgCl_2$, 4 m*M*; KH_2PO_4, 8 m*M*; adenosine triphosphate, 1 m*M*; ethylene-diamine tetra-acetic acid, 1·5 m*M*; sucrose, 200 m*M*; glucose, 30 m*M*; tris buffer of *p*H 7·6, 67 m*M*; potassium ascorbate, 20 m*M*; yeast hexokinase, *c.* 14 units/ml., and riboflavin monophosphate, 0·1 mgm./ml. Reactions were carried out at 10° C. in the air

and may be a more suitable substrate for the reaction than, the externally added citrate or reduced pyridine nucleotides. The inactivity of the whole chloroplasts in the oxidative phosphorylation may be explained by the impermeability of the added substrate to the site of the reaction.

I thank Prof. S. Funahashi for his advice and interest in this work.

Takao Ohmura
Department of Agricultural Chemistry,
University of Tokyo.
Tokyo, Japan.
May 15.

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Effect of Inhibition of Cholinesterase on the Motility of the Small Intestine

Experiments with anticholinesterases have shown that the pseudocholinesterase of rat, dog and man can be inhibited without eliciting any symptoms of acetylcholine accumulation in the intact animal[1]. On the other hand, inhibition of the true cholinesterase by more than 80 per cent leads to acute disturbances indicative of acetylcholine poisoning, while complete inhibition of the true cholinesterase activity is invariably fatal. These results support the original classification of the cholinesterases according to physiological function as proposed by Mendel and Rudney[2], who surmised that the destruction of acetylcholine in the body, wherever it takes place rapidly, is brought about mainly, if not exclusively, by the true cholinesterase.

Later experiments indicated that the contractions of the longitudinal muscle of the small intestine were increased by di*iso*propyl phosphonofluoridate in amounts which did not cause more than 20 per cent inhibition of the true cholinesterase while inhibiting the pseudocholinesterase almost completely[3]. These results seemed to indicate that the pseudocholinesterase is concerned with the hydrolysis of acetylcholine in the muscle of the small intestine. However, Shelley[4] recently reported results which suggest that the pseudocholinesterase does not play an important part in the destruction of acetylcholine in the isolated longitudinal muscle coat of the rabbit intestine *in vitro*. She found that the increase in the tone of the intestinal muscle ran parallel with the percentage inhibition of the true cholinesterase by eserine and by di*iso*propyl phosphonofluoridate, regardless of the degree of inhibition of the pseudocholinesterase effected by these compounds. The increase in tone in the longitudinal muscle first appeared when less than 20 per cent of the true cholinesterase was inhibited, and reached a maximum with total inhibition of this enzyme.

The results obtained in our experiments on the motility of the small intestine of the cat under more physiological conditions have been briefly reported previously[5] and confirm those obtained *in vitro* by Shelley[4]. For these experiments, a method was designed by which the contractions of a segment of the small intestine with intact blood supply could be measured *in situ*, and the changes in intestinal motility and cholinesterase activities were determined after intravenous injections of three anticholinesterases: di*iso*propyl phosphonofluoridate (DFP), tetraethyl pyrophosphate (TEPP) and the prostigmine analogue (*Nu* 1250) described by Hawkins and Mendel[6]. These investigations also indicated that the pseudocholinesterase is not of primary importance for the hydrolysis of the acetylcholine formed in the small intestine, while there is, on the other hand, a fairly close correlation between the degree of inhibition of the true cholinesterase activity by the various compounds and their effects on the intestinal motility.

In confirmation of Shelley's findings, the motility of the longitudinal muscle was increased when the true cholinesterase was inhibited by about 20 per cent. The circular muscle appeared to be somewhat less sensitive to all three anticholinesterases and required about 40 per cent inhibition of the true cholinesterase before its motility was increased. In confirmation of previous observations on intact animals[6], it was found that peristaltic movements appear when the true cholinesterase is more than 80 per cent inhibited. This difference may be explained by the fact that the normal rhythmic movements of the small intestine are due, to a large extent, to acetylcholine of non-nervous origin, whereas the peristaltic movements depend on synaptic transmission in the myenteric plexus[7].

It is conceivable that there is a continuous equilibrium between the release and hydrolysis of acetylcholine in the muscle of the small intestine which

is affected by a relatively small inhibition of the true cholinesterase activity. On the other hand, a small inhibition of the true cholinesterase appears to have no measurable influence on the transient depolarization produced by the intermittent release of acetylcholine at the synapses in the central nervous system, myenteric plexus and ganglia and at the myoneural junctions in the skeletal muscles. As suggested by Shelley[4], the high acetylcholine content and the relatively low cholinesterase concentration in the longitudinal muscle of the small intestine may also be important factors in determining the high sensitivity of this tissue to inhibition of the true cholinesterase.

J. Admiraal
D. K. Myers
J. C. van Houten

Pharmaco-therapeutic Laboratory,
University of Amsterdam.
April 27.

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Concentration of *Brucella* Vaccine

Frozen-dried *Brucella* vaccine is in various countries distributed in multiple-dose containers for economy of production and convenience in use. The number of viable, normal organisms per ampoule determines the number of subcutaneous doses one ampoule contains[1].

The dosage per ampoule may be raised by a simple, inexpensive and labour-saving technique.

Sodium-carboxy-methyl-cellulose as a substance with protective qualities which does not encourage the growth of micro-organisms had given some promise as preservative in viable liquid *Brucella* vaccine[2]. It was also found to be an agglutinant for *Brucella* organisms. Combination of these two properties suggested its use for concentrating suspensions of organisms before freeze drying.

Carboxy-methyl-cellulose (commercial, high viscosity, type '70') was added to the *Brucella* vaccine suspension in the form of a sterile watery solution and at the rate of 16 gm. per 4,000 ml. containing approximately 6×10^{14} viable cells[3]. After agitation in a 5-litre flask, this was held at 4·0° C. for 18 hr. to allow for sedimentation of the cells. The supernatant was siphoned off and *mist. desicans* was added to the sediment. The mixture was pipetted into ampoules and freeze-dried on an Edwards Model No. 3 apparatus.

Pre-drying counts showed $1,375 \times 10^9$ viable organisms per ml., and a 50 per cent survival-rate was obtained when the ampoule was sealed in pure nitrogen after freeze-drying. One half ml. dried in a 1·0 c.c. ampoule thus contained 337×10^9 organisms or five doses of vaccine by the conventional standards.

Carboxy-methyl-cellulose is harmless, inert and commonly taken internally. Parenterally it produced no effect in 0·1 per cent solution under the skin. Biological reactions of the concentrated vaccine in guinea pigs and cattle have given results identical to those obtained with vaccine made from agar surface culture. It was tested only in the prescribed density of 12×10^9 organisms per ml.

Acknowledgments are made to Dr. R. A. Alexander, director of veterinary services, and to technical assistants Mr. F. D. Horwell, Mr. G. du Plessis and Miss H. E. Smith.

G. C. van Drimmelen

Department of Bacteriology,
Faculty of Veterinary Science,
University of Pretoria,
and
Section of Bacteriology,
Veterinary Research Laboratory,
Onderstepoort.

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Hæmoglobin *E* in Indonesia

Five abnormal forms of human hæmoglobin, hæmoglobin *S*, *C*, *D*, *E* and *G*, have been described in addition to the normal adult (*A*) and fœtal (*F*) hæmoglobins.

Hæmoglobin *E*, which until recently was thought to be rare, has been found to have a high incidence among the Thais[1]. Its pattern of incidence may be of much anthropological importance. In the present communication I am now reporting the finding of hæmoglobin *E* among the Indonesian peoples.

In a previous communication, cases of Cooley's anæmia in Indonesian children have been described[2]. In order to identify the exact nature of this congenital anæmia, I made an electrophoretic study of the hæmoglobin in the erythrocytes of these individuals. At the same time a survey of pathological hæmoglobins among Indonesians was carried out.

The method employed was a slight modification of that described by Smith and Conley[3] using veronal buffer *p*H 8·6 and ionic strength 0·05. The hæmoglobin solutions were run 8–14 hr. at room temperature.

Five hundred samples of blood taken at random from patients at the Central General Hospital in Djakarta were examined. Most of the patients were from west Java, and the rest were from different parts of Indonesia; 479 were Indonesians, 13 were of mixed Chinese and Indonesian blood, and 8 were Indo-Europeans. Eighteen individuals showed a pathological hæmoglobin in addition to normal adult hæmoglobin. This pathological hæmoglobin has exactly the same electrophoretic mobility as the hæmoglobin *E* we received from Thailand (hæmoglobin *C* is slower and hæmoglobin *S* migrates faster). Its visible and ultra-violet spectra were the same as those of adult hæmoglobin. I therefore diagnosed hæmoglobin *E*.

Of the eighteen individuals carrying hæmoglobin *E* in their blood, sixteen were Indonesians, one was of mixed Chinese and Indonesian blood and one was an Indo-European. The incidence among the 500 individuals examined was therefore 3·6 per cent. No other pathological hæmoglobins were found electrophoretically.

Hæmoglobin solutions prepared from the erythrocytes of cases described as Cooley's anæmia in my earlier paper[2] were also examined. Two of the cases have only adult and fœtal hæmoglobin, which is the pattern expected in Cooley's anæmia. The third case

(case 2 in our publication, whose father was no longer alive for observations) was a case of Mediterranean-hæmoglobin *E* disease. In addition, two other cases of Mediterranean-hæmoglobin *E* disease were discovered.

In those carrying the trait, hæmoglobin *E* formed the minor component, whereas in Mediterranean-hæmoglobin *E* disease the pathological hæmoglobin was the major component (judged from the intensity of the spots on the paper). This fact was also observed by Chernoff *et al.* in their study of hæmoglobin *E* in Thailand. There was no increase in fœtal hæmoglobin (by Singer, Chernoff and Singer's method[4]) in any of the carriers of the trait examined.

Lehman (personal communication) recently found two carriers of the hæmoglobin *E* trait among people from Ceylon.

It is probable that hæmoglobin *E* is prevalent in South-East Asia, and a study of its incidence in different areas of this part of the world is of anthropological interest.

This work is continuing, and a more complete report will be made in the future.

Drs. Itano, Bergren and Sturgeon, who kindly made a comparative study of my pathological hæmoglobin with their own hæmoglobin *E*, found that the former is exactly the same as the original hæmoglobin *E* described by them in 1954. Dr. Itano established the identity by moving-boundary electrophoresis both at *p*H 6·5 and 8·6.

Lie-Injo Luan Eng

Department of Parasitology and
General Pathology,
University of Indonesia. March 25.

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Predation Pressure and Evolution in Lake Nyasa

In papers dealing with the evolution of fishes in the Great Lakes of East and Central Africa, Worthington[1-3] postulates that a major contributory factor to the great adaptive radiation shown by the family Cichlidae in certain lakes, notably Victoria, Edward and Nyasa, has been the absence from their waters of active predators. The theoretical implications of this suggestion have been accepted by Huxley[4] but have been criticized by Mayr[5].

Worthington's supposition is that large and vigorous predaceous fishes of the genera *Lates* and *Hydrocyon* which are present in several African lakes have played an important part in restricting the adaptive radiation of the non-predaceous species. In Lake Nyasa, where these genera are not represented, he claims that their place is taken by a much less vigorous type of predator in the form of two species of *Barilius*, and as a result large-scale radiation has been permitted to occur.

The results of recent work by us on the ecology and feeding habits of fishes inhabiting both the inshore regions and open waters, together with the results of the 1939 fishery surveys[6] and the work of Lowe[7], indicate that the restrictive effects of predation are not so important as Worthington suggests, as it is now clear that Lake Nyasa harbours a large number of markedly predaceous fishes, each major habitat having its complement of predators. Although a difference in degree of predation exerted by *Lates* and *Hydrocyon* and by the Nyasan predators is not denied, this is probably compensated for by the great numerical abundance of some of the Nyasan species.

Predaceous species of the family Cichlidae include the following: *Rhamphochromis* (at least eight species), the dentition and diet of which are similar to that occupied by the pikes of Europe and North America; *Serranochromis thumbergi* (Castelnau), *Aristochromis christyi* Trewavas, and several species of *Haplochromis*, for example, *H. kiwinge* Ahl, *H. dimidiatus* (Günther) *H. compressiceps* (Boulenger) and *H. macrostoma* Regan. The non-cichlid piscivorous species include *Bagrus meridionalis* Günther, *Barilius microcephalus* Günther, *Barbus rhoadesii* Boulenger, and several species of *Clarias*.

Many of these species feed exclusively or almost exclusively on fishes except, of course, when very young. In addition, many other species living in the Lake can be classed as mild predators which take fish as part of their diet. Further, it has been observed that almost any fish in the Lake, be it carnivore or herbivore, will take the eggs or fry of other fishes whenever the opportunity to do so arises.

Fishes in Lake Nyasa, therefore, do not lead lives which are relatively uninfluenced by predators as Worthington suggests. On the contrary, the percentage of predators appears to be extremely high, and as it is improbable that all the predators represent the most recently evolved species there is no reason to suppose that, except perhaps in the very early stages of colonization, the Lake has ever been without its complement of predaceous species.

The fact that we cannot agree with the suggestion that the great adaptive radiation shown by the Nyasan fish fauna has taken place largely because of the absence of predators does not mean that we adopt Mayr's view that the phenomenon can be explained on the basis of succession of colonizations from a number of different river systems. This, while it may have played some part in the initial stages of the process can certainly be discounted as an important cause of the state of affairs which exists to-day. The purpose of the present note, however, is not to propose new theories but to indicate that a previously widely circulated view can no longer be held.

G. Fryer
T. D. Iles

Joint Fisheries Research Organization,
Nkata Bay, Nyasaland.

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So-called Endosperm Embryos in *Isomeris arborea* Nutt

In 1937 Billings[1] published a paper on the reproductive cytology of angiosperms, illustrated by *Isomeris arborea*. His observations may be summarized as follows: (1) this plant is a haploid with a complement of 17 chromosomes; there is no true meiosis or syngamy; (2) the megaspore mother cell

functions directly without producing the usual megaspores; (3) the mature embryo sac is 3-nucleate, consisting of two synergids and an endosperm nucleus, a true egg being absent; (4) there is no fertilization; (5) the endosperm begins as free nuclear, but soon a part of it becomes differentiated into multinucleate vesicles or nodules; and (6) the embryo arises as a direct outgrowth from one of the endosperm nodules.

From time to time, other embryologists have expressed doubt on the accuracy of these reports and stressed the need for a re-investigation of *Isomeris*[2-4]. In recent years Maheshwari and Khan[5], and Maheshwari and Sachar[6] were able to rectify some of the errors made by Billings. Maheshwari and Khan[5] have clearly shown that the megaspore mother cell undergoes the normal reduction divisions and forms a 'triad' consisting of an upper dyad cell and two megaspores. The chalazal megaspore gives rise to an 8-nucleate embryo sac of the *Polygonum* type. Billings had assumed the absence of an egg cell and the formation of the embryo from one of endosperm nodules located somewhere in the lower part of the embryo sac. We found a perfectly normal egg cell and an embryo in the usual micropylar position.

A few points remained nevertheless to which a clue has been obtained only recently. These are: (*a*) whether fertilization is really absent; (*b*) what is the nature of primary endosperm nucleus; (*c*) what is the true origin and function of endosperm nodules, and (*d*) what is it that brings the embryo to the basal part of the embryo sac cavity?

Billings stated: "the endosperm nucleus in *Isomeris arborea* is single in origin and is haploid with respect to its chromosome complement . . .". This is not true, as we have found two polar nuclei lying side-by-side in the upper part of the embryo sac. Actual fusion could not be observed; but the size and appearance of the large central nucleus in many embryo sacs leaves no doubt that it represents the secondary nucleus.

Billings saw pollen tubes in many ovules, but surmised that the sperms were never discharged. Fig. 1 here clearly shows syngamy as well as triple fusion, and it may therefore be concluded that the embryo and the endosperm are diploid and triploid respectively, as is typical of most angiosperms. Since Billings never observed double fertilization, he wrongly inferred that the endosperm is haploid.

The zygote, while still at the micropylar position, undergoes a series of transverse divisions and forms a long uniseriate pro-embryo (Figs. 2–4). Later, all the cells of the suspensor enlarge considerably in size and also become multinucleate (Figs. 8, 9). Their hypertrophied nature suggests a haustorial role. Due to the formation of an exceptionally long suspensor the embryo proper is pushed down in the lower part of the embryo sac. A study of whole mounts of embryo sacs dissected out from ovules was very useful and revealed features which had escaped notice in sections. It is now clear that the long suspensor often becomes coiled and twisted (Fig. 10). In microtome sections this organ is cut up into several segments which appear as multinucleate vesicles imbedded in the free nuclear endosperm. Billings, who had missed both the zygote as well as the young pro-embryo, was led to believe that these vesicles or nodules are derived from the free nuclear endosperm; and as he saw a close connexion between these vesicles and the embryo proper in the middle part of the embryo sac, he postulated an endospermic origin of the embryo (Fig. 11). It is now evident that these vesicles, which were also provisionally accepted by us as endosperm nodules, are in reality the cells of the massive suspensor and have nothing to do with the endosperm (Figs. 5–7).

Briefly then, contrary to what Billings had said, *Isomeris arborea* follows the usual course of development followed in angiosperms, fertilization occurs normally, and the embryo does not arise from an

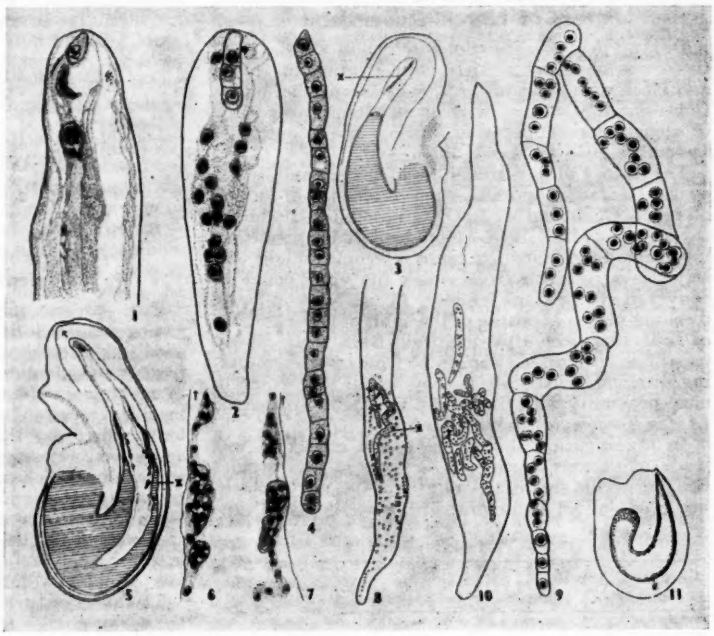

Fig. 1. Upper part of embryo sac showing syngamy and triple fusion; note a portion of the pollen tube below the fertilized egg. (×200)
Fig. 2. Embryo sac with 3-celled pro-embryo at the micropylar end and free endosperm nuclei. (×200)
Fig. 3. Diagrammatic representation of l.s. ovule showing long pro-embryo (*x*) and free nuclear endosperm represented by dots. (× 20)
Fig. 4. Magnified view of the filamentous pro-embryo in Fig. 3. (× 200)
Fig. 5. L.S. ovule showing endosperm nodules in middle region of embryo sac. (× *c.* 20)
Fig. 6. Part of embryo sac enlarged to show endosperm nodules. (× 150)
Fig. 7. Part of embryo sac enlarged to show embryo marked *x* in Fig. 5. (× 150)
Fig. 8. Whole mount of lower part of embryo sac showing embryo (*x*) and free endosperm nuclei. (× 50)
Fig. 9. Magnified view of embryo in Fig. 8 to show the hypertrophied multinucleate cells of the suspensor and three uninucleate terminal cells. (× 218)
Fig. 10. Whole mount of embryo sac showing pro-embryo with massive coiled suspensor (endosperm nuclei not shown). (× 50)
Fig. 11. Reproduced from Billings (ref. 1) to show the endosperm nodules along the wall of the embryo sac

endosperm nodule. The really interesting feature in the embryology of *Isomeris arborea* is the formation of a massive suspensor haustorium. This consists of densely cytoplasmic multinucleate cells which were misinterpreted by Billings as endosperm nodules.

Thanks are due to the Indian Council of Agricultural Research for a research grant in connexion with a scheme on "Chemical Stimulation of the Ovule", and to Mr. S. C. Maheshwari for some useful suggestions in the interpretation of the nature of the endosperm and embryo.

P. Maheshwari
R. C. Sachar

Botany Department,
University of Delhi,
Delhi 8, India.
July 27.

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Relationships between the Flying Fox (Genus *Pteropus*) and Arthropod-borne Fevers of North Queensland

During field investigations by one of us (L. C. R.) there was observed a fairly close relation in season and place with areas of the city of Townsville, North Queensland, Australia, in which an extensive epidemic of dengue fever occurred during the months of December 1953 to March 1954.

The history of past epidemics in Queensland, so far as can be traced, and also observations made by two of us (L. C. R. and J. L. O'C., unpublished) during the course of the 1953–54 epidemic suggested that members of the flying fox genus (*Pteropus*) might well serve as a reservoir from which the virus could be conveyed to regions closely in association with dwelling-places—because of the habit of these animals of swarming, just after nightfall, from their camps, usually in mangrove swamps, to fruit trees surrounding dwelling places, where they would thus come in close proximity with the known vectors in Townsville (*Aedes* (*Stegomyia*) *aegypti* Linn.).

In investigating this hypothesis a collection was made of various species of *Pteropus* in and around the Townsville area in the months of October–November 1954—about two months prior to the usual time of onset of dengue fever. It should be observed that only a few sporadic cases occurred in the city over the period December 1954–November 1955.

Table 1

Serial No.	Species	Deaths in mice*	Presence of antibody
FF No. 1	*gouldi*	0/6	positive
2	,,	5/6	negative
3	*scapulatus*	5/6	,,
4	,,	2/6	inconclusive
5	,,	3/6	,,
6	,,	4/6	,,
7	,,	3/6	,,
8	*gouldi*	4/6	,,
9	,,	6/6	negative
10	*scapulatus*	3/6	inconclusive
11	,,	6/6	negative
12	,,	6/6	,,
13	,,	5/6	,,
14	*gouldi*	0/6	positive
15	,,	0/6	,,
16	,,	3/6	inconclusive
17	,,	1/6	positive

* The numerator indicates the number of mice dead at the end of the test; the denominator indicates the number of mice inoculated.

Specimens of two species were obtained: sixteen of *P. gouldi* and eighteen of *P. scapulatus*. The brains of these were submitted to histological examination, and heart blood to serological test for protective antibodies against the virus of dengue fever Type I. Mice were inoculated with a mixture of equal volumes of inactivated undiluted serum and virus suspension containing 100 mouse *LD* 50. Mixtures were incubated for two hours at 37° C. prior to injection into six mice for each serum specimen.

The results of such protection tests are set out in Table 1.

Those tests in which five or six mice survived are taken as positive evidence of the presence of protective antibodies, those in which 0 or 1 mouse survives are interpreted as evidence of the absence of antibody. Tests in which 2, 3 or 4 mice survived are regarded as inconclusive.

Although the challenge virus used was dengue Type I (the Hawaiian strain isolated by Sabin), these results are not interpreted as conclusive of past infection by that virus.

Current work by us indicate that there is a very close serological relationship between the virus of dengue fever (Type I) and of Murray Valley encephalitis.

J. L. O'Connor
L. C. Rowan
Commonwealth Serum Laboratories,
Melbourne.
J. J. Lawrence
School of Public Health and Tropical Medicine,
University of Sydney,
New South Wales.

A Variable Dosage Sprayer for Agricultural Experiments

The present method for testing in the field the toxicity and selectivity of herbicides or other spray chemicals necessitates spraying a number of separate plots with different dosages. Valuable time is wasted in making up solutions and emptying the spraying machine between operations. The time required is longer the closer the gradations of dosage which it is considered necessary to examine. We have designed a machine which makes it possible with one filling to spray continuously decreasing concentrations, starting from an arbitrary upper limit. A whole spectrum of results is thus made available for examination in far less time than is consumed in getting less complete results by the established method.

The dilution principle used is to pump liquid to the spray nozzles from an intermediate vessel which is otherwise closed except for a lead tube from a water-supply vessel, is equipped with an efficient agitator and is initially completely filled. As spray liquid is withdrawn it is replaced by water and the concentration falls in an exponential manner. A coarsely porous plug in the entrance tube of the mixing vessel prevents adventitious dilution. The nozzles are carefully selected to have equal output at a given pressure, and are fed by separate narrow polythene tubes of equal length from a common manifold as close as possible to the pump, so that the same concentration reaches all nozzles simultaneously.

The pump is directly geared to the tractor on which the machine is mounted, and therefore the concentration taken into the pump is an exponential function of distance along the plot. Since it is necessary for air to be expelled from the spray system before the plot is reached, the simple principle above described is modified by providing also a direct tube from the water tank to a three-branch cock immediately above the pump. The tractor is driven towards the plot spraying water only, pumped through this line. The cock is then quickly thrown over and spraying of chemical commences. Owing to residual water in the pump and manifold, the concentration at the spray nozzles, after clearance of the tubes to the nozzles, rises steeply to a concentration somewhat less than that initially charged, before decreasing along the remaining length of the plot in an approximately exponential manner. The concentration is uniform across the plot. The form of the curve can be worked out theoretically from the volumes; but results are based on experimental calibration. The experimental curve is very accurately constant over the range of speed of a tractor working in one gear. Since the machine is effectively self-washing, repeated sprayings with the same or different chemicals can be made very rapidly.

The machine has enabled the expected advantages of speed and continuity to be obtained, and in addition provides a means of obtaining a rapid estimation of selectivity, and the dosage ratio for limiting crop-damage and limiting weed-kill. Changes in apparent effect with time are also easily observed. By filling throughout with the same spray liquid, the machine can be used for normal spraying. It is also possible to use the machine for examining the effect of admixture of herbicides or of a herbicide and an ancillary (for example, wetting) agent in continuously varying ratio. The exponential form for one concentration or ratio is not, however, the most satisfactory for this application, and a modified machine, using two mixing tanks in series, which gives a sigmoid concentration–distance curve, is to be preferred.

Our thanks are due to Mr. A. Ross for construction of the machine and to the directors of Fisons Pest Control Ltd. for permission to publish this account.

R. Pfeiffer
R. T. Brunskill
G. S. Hartley

Chesterford Park Research Station,
Near Saffron Walden,
Essex. July 14.

Live Coelacanths

From the discovery of the first Coelacanth at East London in 1938, it was my aim not only to discover their true home, but also I hoped to live to see a living Coelacanth, and for mankind generally to be able to see this living link with the incredibly remote past. When *Malania* was found at the Comores, I planned to catch Coelacanths alive and to keep them alive. It is therefore gratifying that the French are plainly making every endeavour to achieve this. The article by Prof. J. Millot in *Nature* of February 26, 1955, on the experience of the first living Coelacanth at the Comores is of special interest.

The failure of the French to keep their fish alive for more than a few hours is attributed by them to decompression combined with rise in temperature of the water, while a high degree of photophobia on the part of the fish is alleged. While there may be something in this, in my view the cause is probably quite different. Prof. Millot and his collaborators are possibly not aware of the experience that large fishes taken alive after a struggle on a line even with no visible laceration rarely live long after, certainly not in aquaria, and even when liberated many die very shortly. Curiously enough, fishes taken by harpooning, even when extensively gashed, show a greater survival-rate than those taken on hooks. Coelacanths caught by net or trap and kept in a closed vessel will almost certainly have a greater chance of survival even at normal pressure.

It is doubtful whether the view about decompression or small variation in temperature is tenable, since after being hauled to the surface in a trawl-net near East London, the first Coelacanth lived for more than three hours, out of the water, on the deck of a trawler on an unusually warm day.

It is interesting to note that the French used a boat as an improvised aquarium. At a meeting in Nairobi in October 1953 I suggested a decked boat, since it seemed likely that an important factor in survival would be to shield the fish from shocks until such time as it could become accustomed to a new environment. An open whaler, however, was used at Matsumudu, so that the fish had a clear view. We are told that "Throughout the night, which the delighted population of Matsumudu passed in singing and dancing to celebrate the capture, the Coelacanth was watched over with admirable care", by officials, doubtless with constantly flashing torches, and only those who have experienced a night such as is indicated here can have any idea of the noise and lights: the Coelacanth at Matsumudu must have passed the night in a state of high nervous tension.

What the French considered 'photophobia' on the part of the Coelacanth is, in my view, no more than the natural uneasiness that any large and intelligent fish would experience as unfamiliar surroundings and objects become increasingly obvious from dawn.

The 'luminescence' of the eyes of the live Coelacanth is interesting. This phenomenon is, however, quite common in sharks and other large fishes of shallow waters, and on this night there was bright moonlight.

It is a notable feature of the reports that the depth to a metre at which each Coelacanth was caught has been stated. As all these were apparently taken by natives fishing from drifting canoes at night, and the slope of the bottom offshore at the Comores is stated to be at least 50°, it would be of general interest to know how this high order of accuracy is achieved.

In the matter of the first egg-bearing female Coelacanth, it is a strange coincidence that this was captured by one of the crew of Captain Hunt's vessel, only a short distance from where this was anchored. (It was Captain Hunt who took our Coelacanth leaflets to the Comores, which resulted in the discovery of *Malania* there.) The fish was apparently cut open and seen to contain a cluster of eggs at all stages of development "such as is observed in a fowl", or in oviparous sharks. We may therefore expect Coelacanths to have egg-cases like those of Elasmobranchs.

J. L. B. Smith

Rhodes University,
Grahamstown,
Cape Province.
June 17.

FORTHCOMING EVENT

Sunday, September 4—Wednesday, September 7

British Association for the Advancement of Science (at Bristol).—Continuation of Annual Meeting.

Sunday, September 4

At 11 a.m.—(in Bristol Cathedral)—Religious Service. Preacher: the Rt. Rev. the Lord Bishop of Bristol.

At 8 p.m.—Prof. H. D. F. Kitto: "Scientific and Dramatic Modes of Thought".

Monday, September 5

At 10 a.m.—Mr. W. N. Edwards: "Geographical Distribution of Past Floras" (Presidential Address, Section C).

At 10 a.m.—Dr. R. S. Creed: "The Sensory Pathway" (Presidential Address, Section I).

At 8 p.m.—Mr. R. J. C. Atkinson: "Stonehenge in the Light of Recent Research".

Tuesday, September 6

At 2.30 p.m.—Mr. B. J. Mason: "Man's Influence on the Weather: Recent Progress and Future Possibilities" (Special Lecture for Young People).

APPOINTMENTS VACANT

Applications are invited for the following appointments on or before the dates mentioned:

Assistant Physicist (Basic grade) at the Regional Radium Institute, Bradford—The Secretary, Royal Infirmary, Bradford (September 12).

Tutor in Geography—The Registrar, The University, Liverpool (8 September 12).

Scientific Officer (with an honours degree in chemistry) for scientific control and development at Northern Outfall Works, Beckton, E.6—The Medical Officer of Health (PH/D1), County Hall, London, S.E.1 (September 14).

Lecturer in Pharmacology at the University of Hong Kong—The Secretary, Association of Universities of the British Commonwealth, 36 Gordon Square, London, W.C.1 (September 15).

Physicists and Mechanical Engineers (with a first- or second-class honours degree in physics or mechanical engineering, and preferably with some understanding of aerodynamics) at the Royal Aircraft Establishment, Farnborough, Hants, for research and development mainly concerning engineering, instrumentation and problems of parachute development and application, vulnerability of aircraft, refrigeration and air conditioning—The Ministry of Labour and National Service, Technical and Scientific Register (K), 26 King Street, London, S.W.1, quoting A.304/5A (September 17).

Senior Research Officer or Research Officer (with a university honours degree in science or fuel technology) in the Coal Research Section, Commonwealth Scientific and Industrial Research Organization, North Ryde, New South Wales, Australia, to participate in the studies being undertaken to evaluate combustion characteristics of Australian coals—Chief Scientific Liaison Officer, Australian Scientific Liaison Office, Africa House, Kingsway, London, W.C.2, quoting 480/154-2 (September 24).

Lecturer or Assistant Lecturer in Metallurgy—The Registrar, The University, Leeds 2 (September 26).

Assistant Lecturer in Dairy Bacteriology—The Registrar, University College of Wales, Aberystwyth (September 30).

Lecturer in Mechanical Engineering—The Registrar, College of Technology, Manchester 1 (September 30).

Junior Research Fellow (with a physics degree) in the Instrument Technology Section of the Physics Department, for photoelectronic research—The Professor of Instrument Technology, Imperial College of Science and Technology, London, S.W.7 (October 1).

Society for Analytical Chemistry Research Fellow (with at least an honours degree or A.R.I.C., and with experience in analytical chemistry) in Analytical Chemistry, for work concerned with the determination of elements by means of radiochemical methods—The Secretary of the Analytical Methods Committee, The Society for Analytical Chemistry, 7–9 Idol Lane, London, E.C.3 (October 1).

Chair of Cultural Anthropology at the University of the Witwatersrand—The Secretary, Association of Universities of the British Commonwealth, 36 Gordon Square, London, W.C.1 (South Africa, October 10).

Lecturer in Philosophy at Auckland University College, New Zealand—The Secretary, Association of Universities of the British Commonwealth, 36 Gordon Square, London, W.C.1 (New Zealand, October 15).

Lecturer in Physics at Victoria University College, Wellington, New Zealand—The Secretary, Association of Universities of the British Commonwealth, 36 Gordon Square, London, W.C.1 (New Zealand, October 15).

Chair of Engineering at the University of Tasmania—The Secretary, Association of Universities of the British Commonwealth, 36 Gordon Square, London, W.C.1 (Tasmania, October 30).

Senior Lecturer in Bacteriology and an Assistant Lecturer in Bacteriology at the University of Hong Kong—The Secretary, Association of Universities of the British Commonwealth, 36 Gordon Square, London, W.C.1 (Hong Kong, October 31).

Assistant Experimental Officer (with at least G.C.E. (advanced level) or intermediate B.Sc. with mathematics) as Assistant to Statistician, for computational duties and some photographic work—The Director, Anti-Locust Research Centre, c/o British Museum (Natural History), Cromwell Road, London, S.W.7.

British Steel Castings Research Association Fellow (graduate, with some research experience) to work under the direction of Dr. T. P. Hoar in the Department of Metallurgy at Cambridge University, on problems relating to steel castings—Dr. T. P. Hoar, The Department of Metallurgy, Pembroke Street, Cambridge.

Chair of Physiology at Makerere College (University College of East Africa)—The Secretary, Inter-University Council for Higher Education Overseas, 1 Gordon Square, London, W.C.1.

Group Leader (Nuclear Physics Branch) to develop specialized electronic circuits for nuclear physics research in an expanding Branch whose facilities include a 6 MeV. Van de Graaff generator and other accelerators—The Senior Recruitment Officer, Atomic Weapons Research Establishment, Aldermaston, Berkshire, quoting Ref. 747/34.

Technical Officer (qualified chemist) to act as Secretary to committees preparing standards in the chemical and related fields—The Establishment Officer, British Standards Institution, 2 Park Street, London, W.1.

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

Great Britain and Ireland

Your Weather Service. Prepared by the Meteorological Office and the Central Office of Information. Revised edition. Pp. 52+8 plates. (London: H.M. Stationery Office, 1955.) 1s. 6d. net. [276

The Curtis Museum, Alton, Hampshire, 1855–1955: the First Hundred Years of a Small Museum. By William Hugh Curtis. Pp. vii+55+10 plates. (Alton: Curtis Museum, 1955.) 6s. [276

Plants of the Sea. By Prof. W. E. Isaac. (Inaugural Lecture delivered before the University of Cape Town, April 1955. University of Cape Town Lecture Series—No. 6.) Pp. ii+23. (London: Oxford University Press, 1955.) 3s. 6d. [276

Department of Scientific and Industrial Research. Food Investigation Special Report No. 60: Eggs and Egg Products. By Dr. J. Brooks and D. J. Taylor. Pp. vi+106. (London: H.M. Stationery Office, 1955.) 4s. net. [296

Aero Research Technical Notes. Bulletin No. 149: "Redux"—The First Twelve Years. Pp. 6. (Duxford: Aero Research, Ltd., 1955.) [296

A Critical Study of Electrical Stunning and the Jewish Method of Slaughter (Shechita). By Rabbi Solomon David Sassoon. Pp. 40+2 plates. (Letchworth: Rabbi Solomon D. Sassoon, 15 Sollershott East, 1955.) 2s. 6d. [296

Other Countries

Unified Symbolism for World Understanding in Science, including Bliss Symbols. By Prof. Oliver L. Reiser. (A Paper read in parts at the Annual Meeting of the American Association for the Advancement of Science, Philadelphia, 1951, and at the Conference of the International Society of Significs, Amsterdam, 1953.) Pp. 52. (Sydney: Semantography Publishing Company, 1955.) [276

British Guiana. Report on the Geological Survey Department for the year 1953. Pp. 1+111+14 Figs. (Georgetown: Geological Survey, 1954.) 2 dollars. [276

Union Observatory, Johannesburg, S. Africa. Annual Report for 1952. Pp. 4. (Johannesburg: Union Observatory, 1954.) [276

Report on Drug Addiction. By the New York Academy of Medicine (Committee on Public Health, Subcommittee on Drug Addiction). Pp. 33. (New York: New York Academy of Medicine, 1955.) [276

Commonwealth Observatory, Canberra. Report of the Commonwealth Astronomer for the year 1954. Pp. 8. (Canberra: Commonwealth Observatory, 1955.) [276

Annual Report of the Queen Victoria Museum and Art Gallery for the year ended 1953–1954. (Ninety-seventh year.) Pp. 7. (Launceston: Queen Victoria Museum and Art Gallery, 1955.) [276

Companhia de Diamantes de Angola. (Diamang.) Servicos Culturas. Dundo—Lunda—Angola. Museu do Dundo. Subsidios para a História, Arqueologia e Etnografia dos povos da Lunda. Campanha Etnográfica ao Tchiboco (Alto-Tchicapa): Notas de Viagem. Por José Redinha. Pp. 172+5 plates. (Lisboa: Companhia de Diamantes de Angola, 1953.) [276

Report on the Scientific Results of the *Michael Sars* North Atlantic Deep-Sea Expedition, 1910. Vol. 4, Part 2, No. 4: Iniomi (Myctophidae exclusive), Lyomeri, Apodes from the *Michael Sars* North Atlantic Deep-Sea Expedition 1910. By Einar Koefoed. With a section of the Serrivomeridae by M.-L. Bauchot-Boutin. Pp. 15+2 plates. (Bergen: University of Bergen, 1955.) [296

Commonwealth Scientific and Industrial Research Organization, Australia. Soil Publication No. 4: The Soils of the Macquarie Region, New South Wales. By R. G. Downes and J. R. Sleeman. Pp. 72+1 map. (Melbourne: Commonwealth Scientific and Industrial Research Organization, 1955.) [17

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